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Cover illustration: Distinguished visitors at the 1925 meeting of the Society of Experimental Psychologists. *Left to right:* Wolfgang Köhler, visiting professor, Clark University; Walter B. Pillsbury, University of Michigan; Howard C. Warren, Princeton University; Kurt Koffka, visiting professor, Cornell University; and Edward B. Titchener, Cornell University. The April 1925 meeting at Princeton University provided many American psychologists with their first opportunity to meet Köhler and Koffka, the leading Gestalt psychologists. This formal photograph, commemorating the occasion, was preserved by Titchener. Photograph reproduced courtesy of Rand B. Evans, Department of Psychology, Texas A&M University, College Station. See the essay in this issue by Michael M. Sokal, "The Gestalt Psychologists in Behaviorist America."

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A. ROGER EKIRCH is an associate professor of history at Virginia Polytechnic Institute and State University. He received an A.B. from Dartmouth College (1972) and an M.A. (1974) and Ph.D. (1978) from Johns Hopkins University, where he studied early American history with Jack P. Greene. His publications include *"Poor Carolina": Politics and Society in Colonial North Carolina, 1729–1776* (1981) and articles in the *William and Mary Quarterly* (1977), *Perspectives in American History* (1978), and the *Historical Journal* (1984). In 1981–82 he served as the first Paul Mellon Research Fellow in American History at Cambridge University and as a Fellow Commoner of Peterhouse, Cambridge. He is currently completing a book on the transportation of British convicts to eighteenth-century America.

JOHN HIGHAM is an alumnus of Johns Hopkins University (B.A. in history, 1941) and has taught there since 1973. A

historian of American culture, he has written mostly about immigration and ethnicity. An interest in historiography resulted in *History: Humanistic Scholarship in America* (1965), a history of the American historical profession. A second edition, which he revised to include recent developments in the profession, was published in 1983. The article in this issue was occasioned by an invitation to talk about Herbert Baxter Adams at a gathering of local historians at the Peabody Library, Baltimore, which commemorated the three hundred fiftieth anniversary of the founding of the colony of Maryland.

MICHAEL M. SOKAL, a graduate of the Program in History of Science and Technology at Case Western Reserve University, is a professor of history in the Department of Humanities at Worcester Polytechnic Institute, Worcester, Massachusetts. He has written extensively on the history of American science and the history of psychology, including the introduction to *Psychology: Briefer Course* (1982), published as a volume of the *Works of William James*. Other publications include *An Education in Psychology: James McKeen Cattell's Journal and Letters from Germany and England, 1880–1888* (1981) and, with Patrice A. Rafail, *A Guide to Manuscript Collections in the History of Psychology and Related Areas* (1982). Currently Sokal is preparing a full biography of Cattell; his long-range research interests include a history of psychological testing in the United States.

Articles

- Herbert Baxter Adams and the Study of Local History,
BY JOHN HIGHAM 1225
- The Gestalt Psychologists in Behaviorist America,
BY MICHAEL M. SOKAL 1240
- Barristers and Judges in Early Modern Barcelona:
The Rise of a Legal Elite, BY JAMES S. AMELANG 1264

Research Note

- Great Britain's Secret Convict Trade to America, 1783–1784,
BY A. ROGER EKIRCH 1285

Reviews of Books

GENERAL

- | | | | |
|---|------|--|------|
| HOWARD CLARK KEE. <i>Miracle in the Early Christian World: A Study in Sociohistorical Method.</i> By Stephen Benko | 1292 | PHILIP ABRAMS. <i>Historical Sociology.</i> By Richard Harvey Brown | 1298 |
| ALAN H. JONES. <i>Independence and Exegesis: The Study of Early Christianity in the Work of Alfred Loisy (1857–1940), Charles Guignebert (1857–1939), and Maurice Goguel (1880–1955).</i> By John W. Padberg | 1292 | REUVEN BRENNER. <i>History—The Human Gamble.</i> By Alan Sica | 1299 |
| M. L. BUSH. <i>The European Nobility. Volume 1, Noble Privilege.</i> By Jerome Blum | 1293 | DAVID GOODMAN and MICHAEL REDCLIFT. <i>From Peasant to Proletarian: Capitalist Development and Agrarian Transitions.</i> By A. J. Bauer | 1300 |
| JEROME FRIEDMAN. <i>The Most Ancient Testimony: Sixteenth-Century Christian-Hebraica in the Age of Renaissance Nostalgia.</i> By David Ruderman | 1294 | DONALD M. LOWE. <i>History of Bourgeois Perception.</i> By Peter Loewenberg | 1301 |
| STEVEN OZMENT. <i>When Fathers Ruled: Family Life in Reformation Europe.</i> By Claus-Peter Clasen | 1295 | D. C. M. PLATT. <i>Foreign Finance in Continental Europe and the United States, 1815–1870: Quantities, Origins, Functions, and Distribution.</i> By C. P. Kindleberger | 1302 |
| A. LONDON FELL. <i>Origins of Legislative Sovereignty and the Legislative State. Volume 1, Corasius and the Renaissance Systematization of Roman Law; volume 2, Classical, Medieval, and Renaissance Foundations of Corasius' Systematic Methodology.</i> By J. Russell Major | 1296 | HARTMUT KAEUBLE. <i>Soziale Mobilität und Chancengleichheit im 19. und 20. Jahrhundert: Deutschland im internationalen Vergleich.</i> By Beverly A. Heckart | 1302 |
| JOHN LANGDON BROOKS. <i>Just before the Origin: Alfred Russel Wallace's Theory of Evolution.</i> By Frederick B. Churchill | 1297 | CARL H. PEGG. <i>Evolution of the European Idea, 1914–1932.</i> By F. Roy Willis | 1303 |
| STEVEN SEIDMAN. <i>Liberalism and the Origins of European Social Theory.</i> By Jerrold Seigel | 1297 | DAVID FELIX. <i>Marx as Politician.</i> By Martin Berger | 1304 |
| | | PATRICK BRANTLINGER. <i>Bread and Circuses: Theories of Mass Culture as Social Decay.</i> By Henry Vyverberg | 1305 |
| | | STANLEY COHEN and ANDREW SCULL, editors. <i>Social Control and the State.</i> By John K. Alexander | 1305 |
| | | XAVIER DE MONTCLOS. <i>Les chrétiens face au nazisme et au stalinisme: L'Épreuve totalitaire, 1939–1945.</i> By Lawrence D. Walker | 1306 |

- MICHEL RAGON. *The Space of Death: A Study of Funerary Architecture, Decoration, and Urbanism*; MICHEL VOVELLE. *La mort et l'Occident de 1300 à nos jours*.
By Elizabeth Wirth Marvick 1307

ROSS EVANS PAULSON. *Language, Science, and Action: Korzybski's General Semantics—A Study in Comparative Intellectual History*. By Hans Kellner 1308

ALLEN GUTTMANN. *The Games Must Go On: Avery Brundage and the Olympic Movement*. By Charles P. Kort 1309

ANCIENT

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By Bruce Kraig 1310

CHARLES WILLIAM FORNARA. *The Nature of History in Ancient Greece and Rome*. By Donald Kagan 1311

INGEBORG SCHEIBLER. *Griechische Töpferkunst: Herstellung, Handel und Gebrauch der Antiken Tongefäße*.
By William R. Biers 1311

JOHN V. A. FINE. *The Ancient Greeks: A Critical History*.
By Mortimer Chambers 1312

PATRICE BRUN. *Eisphora-Syntaxis-Stratitika: Recherches sur les finances militaires d'Athènes au IV^e siècle av. J.-C.*
By Samuel Eddy 1312

HYLA A. TROXELL. *The Coinage of the Lycian League*.
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LEONHARD SCHUMACHER. *Servus Index: Sklavenverhör und Sklavenanzeige im republikanischen und kaiserzeitlichen Rom*.
By Bruce W. Frier 1314

MARTIN GOODMAN. *State and Society in Roman Galilee, A.D. 132–212*. By Shaye J. D. Cohen 1315

JO ANN MCNAMARA. *A New Song: Celibate Women in the First Three Christian Centuries*. By Emilien Lamirande 1316

MEDIEVAL

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MARK BUCK. *Politics, Finance, and the Church in the Reign of Edward II: Walter Stapeldon, Treasurer of England*.
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JOACHIM-FELIX LEONHARD. *Die Seestadt Ancona im Spätmittelalter: Politik und Handel*. By Reinhold Schumann 1323

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C. T. ALLMAND. *Lancastrian Normandy, 1415–1450: The History of a Medieval Occupation*.
By John Bell Henneman 1324

MODERN EUROPE

NORMAN L. JONES. *Faith by Statute: Parliament and the Settlement of Religion, 1559*. By Joel Berlatsky 1325

J. I. KERMODE and C. B. PHILLIPS, editors. *Seventeenth-Century Lancashire: Essays Presented to J. J. Bagley*.
By Michael J. Galgano 1326

R. W. HARRIS. *Clarendon and the English Revolution*.
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ROBERT R. DOZIER. *For King, Constitution, and Country: The English Loyalists and the French Revolution*.
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By Barbara J. Harris 1329

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By Warren Roberts 1335

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By Mary Lynn Stewart-Mcdougall 1336

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MICHAEL JABARA CARLEY. *Revolution and Intervention: The French Government and the Russian Civil War, 1917–1919*.
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- HARRY VAN VELTHOVEN. *De Vlaamse Kwestie, 1830-1914: Macht en Onmacht van de Vlaamsgezindheden* [The Flemish Question, 1830-1914: Power and Impotence of the Flemish Group]. By Gerlof D. Homan 1341
- DANIEL H. THOMAS. *The Guarantee of Belgian Independence and Neutrality in European Diplomacy, 1830s-1930s*. By Pierre-Henri Laurent 1342
- FOLKERT POSTMA. *Viglius van Aytta als humanist en diplomaat, 1507-1549* [Viglius van Aytta as Humanist and Diplomat, 1507-49]. By Gordon Griffiths 1343
- J. M. M. DE MEERE. *Economische ontwikkeling en levensstandaard in Nederland gedurende de eerste helft van de negentiende eeuw* [Economic Development and Standard of Living in the Netherlands during the First Half of the Nineteenth Century]. By Franklin Mendels 1344
- BJÖRN ASKER. *Officerarna och det svenska samhället, 1650-1700* [Officeholders and Swedish Society, 1650-1700]. By Leland B. Sather 1344
- PAUL-JOACHIM HEINIG. *Reichsstädte, freie Städte und Königtum, 1389-1450: Ein Beitrag zur deutschen Verfassungsgeschichte*. By Steven Rowan 1345
- PETER NEWMAN BROOKS, editor. *Seven-Headed Luther: Essays in Commemoration of a Quincentenary, 1483-1983*. By Susan C. Karant-Nunn 1346
- JAMES ALLEN VANN. *The Making of a State: Württemberg, 1593-1793*. By Hanns Gross 1347
- STEFAN HARTMANN. *Die Beziehungen Preussens zu Dänemark von 1688 bis 1789*. By Linda Frey 1348
- WOLFGANG SCHIEDER, editor. *Liberalismus in der Gesellschaft des deutschen Vormärz*. By James J. Sheehan 1348
- VOLKER PRESS, editor. *Städtewesen und Merkantilismus in Mitteleuropa*. By Gerald L. Soliday 1349
- WALTRAUD SPERLICH. *Journalist mit Mandat: Sozialdemokratische Reichstagsabgeordnete und ihre Arbeit in der Parteipresse, 1867 bis 1918*. By Guenther Roth 1350
- RICHARD CHARLES MURPHY. *Guestworkers in the German Reich: A Polish Community in Wilhelmian Germany*. By Günther Schulz 1351
- ULRICH HEINEMANN. *Die verdrängte Niederlage: Politische Öffentlichkeit und Kriegsschuldfrage in der Weimarer Republik*. By Edward W. Bennett 1352
- RAYMOND L. PROCTOR. *Hitler's Luftwaffe in the Spanish Civil War*. By R. Dan Richardson 1352
- HORST BOOG et al. *Das Deutsche Reich und der Zweite Weltkrieg. Volume 4, Der Angriff auf die Sowjetunion*. By Dennis E. Showalter 1353
- FRANCESCO BENIGNO. *Il porto di Trapani nel Settecento: Rotte, traffici, esportazioni, 1674-1800*. By Antonio Calabria 1354
- GEOFFREY SYMCOK. *Victor Amadeus II: Absolutism in the Savoyard State, 1675-1730*. By John G. Gagliardo 1355
- Mazzini. *Tra insegnamento e ricerca*; NARCISO NADA. *Guglielmo Moffa di Lizio, 1791-1877: Il contributo di un patriota braidese al Risorgimento nazionale*. By Marion S. Miller 1356
- GUIDO DONNINI. *L'Accordo italo-russo di Racconigi*. By Mary Philip Trauth 1357
- JAROSLAV ŠIDAK. *Kroz pet stoljeća hrvatske povijesti* [Through Five Centuries of Croatian History]. By Wayne S. Vucinich 1357
- EDGAR ANDERSON. *Latvijas Vēsture, 1920-1940: Ārpolitika* [History of Latvia, 1920-40: Foreign Affairs]. Volume 1. By Andrejs Plakans 1358
- I. S. DOSTIAN et al., editors. *Pervoe serbskoe vosstanie 1804-1813 gg. i Rossiia* [The First Serbian Revolt of 1804-13 and Russia]. Volume 2, 1808-1813. By David Mackenzie 1359
- LEWIS H. SIEGELBAUM. *The Politics of Industrial Mobilization in Russia, 1914-17: A Study of the War-Industries Committees*. By Evan Mawdsley 1360
- I. I. MINTS. *God 1918i* [The Year 1918]. By Samuel A. Oppenheim 1360
- IU. P. BOKAREV. *Biudzhethnye obsledovaniia krest'ianskikh khoziaistv 20-kh godov kak istoricheskii istochnik* [Peasant Household Budget Studies of the 1920s as Historical Source Material]. By James R. Millar 1361
- D. C. B. LIEVEN. *Russia and the Origins of the First World War*. By Paul Kennedy 1362
- ROBERT L. HUTCHINGS. *Soviet-East European Relations: Consolidation and Conflict, 1968-1980*. By Ellsworth Raymond 1362

NEAR EAST

- B. F. MUSALLAM. *Sex and Society in Islam: Birth Control before the Nineteenth Century*. By Carl F. Petry 1363
- CATHERINE KAMINSKY and SIMON KRUK. *Le nationalisme arabe et le nationalisme juif*. By C. Ernest Dawn 1364
- S. D. GOITEIN. *A Mediterranean Society: The Jewish Communities of the Arab World As Portrayed in the Documents of the Cairo Geniza. Volume 4, Daily Life*. By M. S. Stern 1365
- JOHN J. MCTAGUE. *British Policy in Palestine, 1917-1922*. By Ann M. Lesch 1366
- JACQUES KORNBORG, editor. *At the Crossroads: Essays on Had Ha-Am*. By Stuart A. Cohen 1366
- GUIYU NASHAT. *The Origins of Modern Reform in Iran, 1870-80*. By Mangol Bayat 1367
- GENE R. GARTHWAITE. *Khans and Shahs: A Documentary Analysis of the Bakhtiari in Iran*. By Guity Nashat 1368
- FUAD I. KHURI. *Tribe and State in Bahrain: The Transformation of Social and Political Authority in an Arab State*. By Christine Moss Helms 1368

AFRICA

- MICHAEL MCCARTHY. *Dark Continent: Africa As Seen by Americans*. By Willard B. Gatewood, Jr. 1369
- CLAIRE C. ROBERTSON and MARTIN A. KLEIN, editors. *Women and Slavery in Africa*. By Patricia Romero Curtin 1370
- MICHAEL M. LASKIER. *The Alliance Israélite Universelle and the Jewish Communities of Morocco: 1862-1962*. By David E. Gardinier 1371
- ELIAS N. SAAD. *Social History of Timbuktu: The Role of Muslim Scholars and Notables, 1400-1900*. By Patrick Manning 1371
- D. J. E. MAIER. *Priests and Power: The Case of the Dente Shrine in Nineteenth-Century Ghana*. By Louis Brenner 1372
- JANE L. PARPART. *Labor and Capital on the African Copperbelt*. By Robert Kubicek 1373

ASIA AND THE EAST

- K. C. CHANG. *Art, Myth, and Ritual: The Path to Political Authority in Ancient China*. By Barry B. Blakeley 1373

- BURTON PASTERNAK. *Guests in the Dragon: Social Demography of a Chinese District, 1895–1946.* By Martin King Whyte 1374
- LEE FEIGON. *Chen Duxiu: Founder of the Chinese Communist Party.* By James E. Sheridan 1375
- RONALD P. TOBY. *State and Diplomacy in Early Modern Japan: Asia in the Development of the Tokugawa Bakufu.* By Conrad Totman 1375
- RICHARD RUBINGER. *Private Academies of Tokugawa Japan.* By Earl H. Kinmonth 1376
- MICHAEL M. YOSHITSU. *Japan and the San Francisco Peace Settlement.* By T. C. Rhee 1377
- MOHAMMAD ISHAQ KHAN. *Perspectives on Kashmir: Historical Dimensions.* By Barbara D. Metcalf 1378
- ROBERT HEUSSLER. *Completing a Stewardship: The Malayan Civil Service, 1942–1957.* By Clive S. Kessler 1379
- GORDON REID. *A Nest of Hornets: The Massacre of the Fraser Family at Hornet Bank Station, Central Queensland, 1857, and Related Events.* By Marian Aveling 1379

UNITED STATES

- HENRY F. DOBYNS. *Their Number Become Thinned: Native American Population Dynamics in Eastern North America.* By William C. Sturtevant 1380
- FRED HOBSON. *Tell about the South: The Southern Rage to Explain.* By Hugh C. Bailey 1381
- JESSIE POESCH. *The Art of the Old South: Painting, Sculpture, Architecture, and the Products of Craftsmen.* By Robert Muccigrosso 1382
- DAVID G. PUGH. *Sons of Liberty: The Masculine Mind in Nineteenth-Century America.* By Nancy F. Cott 1382
- MARILYN FERRIS MOTZ. *True Sisterhood: Michigan Women and Their Kin, 1820–1920.* By Barbara Welter 1383
- LOUIS B. WEEKS. *Kentucky Presbyterians.* By John B. Boles 1384
- PETER GROSE. *Israel in the Mind of America.* By John Snetsinger 1384
- ROBERT J. WILSON III. *The Benevolent Deity: Ebenezer Gay and the Rise of Rational Religion in New England, 1696–1787.* By T. Dwight Bozeman 1385
- LYNNE WITHEY. *Urban Growth in Colonial Rhode Island: Newport and Providence in the Eighteenth Century.* By Bruce C. Daniels 1386
- MARY LOU LUSTIG. *Robert Hunter, 1666–1734: New York's Augustan Statesman.* By Robert C. Ritchie 1387
- RONALD HOFFMAN and PETER J. ALBERT, editors. *Arms and Independence: The Military Character of the American Revolution.* By John Pancake 1387
- EUGENE R. FINGERHUT. *Survivor: Cadwallader Colden II in Revolutionary America.* By Robert M. Calhoon 1388
- PETER CHARLES HOFFER. *Revolution and Regeneration: Life Cycle and the Historical Vision of the Generation of 1776.* By William J. Gilmore 1389
- JOSEPHINE F. PACHECO, editor. *The Legacy of George Mason.* By Kenneth E. St. Clair 1389
- WILLIAM G. MCLOUGHLIN. *Cherokees and Missionaries, 1789–1839.* By Mary Young 1390
- RONALD L. HATZENBUEHLER and ROBERT L. IVIE. *Congress Declares War: Rhetoric, Leadership, and Partisanship in the Early Republic.* By Lance Banning 1391
- HENDRIK BOORAEM V. *The Formation of the Republican Party in New York: Politics and Conscience in the Antebellum North.* By Phyllis F. Field 1391
- WILLIAM B. MCCASH. *Thomas R. R. Cobb (1823–1862): The Making of a Southern Nationalist.* By Michael P. Johnson 1392
- CELIA MORRIS ECKHARDT. *Fanny Wright: Rebel in America.* By Lois W. Banner 1393
- BRIGHAM D. MADSEN. *Gold Rush Sojourners in Great Salt Lake City, 1849 and 1850.* By James B. Allen 1393
- NORMA LOIS PETERSON. *Littleton Waller Tazewell.* By William M. Dabney 1394
- WILLIAM HANCHETT. *The Lincoln Murder Conspiracies.* By Gabor S. Boritt 1395
- HAROLD HOLZER et al. *The Lincoln Image: Abraham Lincoln and the Popular Print.* By William Hanchett 1395
- VICTOR B. HOWARD. *Black Liberation in Kentucky: Emancipation and Freedom, 1862–1884;* JULIET E. K. WALKER. *Free Frank: A Black Pioneer on the Antebellum Frontier.* By Thomas C. Cox 1396
- RANDOLPH B. CAMPBELL. *A Southern Community in Crisis: Harrison County, Texas, 1850–1880.* By Gavin Wright 1397
- ROBERT STEVENS. *Law School: Legal Education in America from the 1850s to the 1980s.* By Willard Hurst 1398
- ELIZABETH JOHNS. *Thomas Eakins: The Heroism of Modern Life.* By William M. Armstrong 1398
- AMOS J. LOVEDAY, JR. *The Rise and Decline of the American Cut Nail Industry: A Study of the Interrelationships of Technology, Business Organization, and Management Techniques.* By Gary L. Browne 1399
- DAVID O. WHITTEN. *The Emergence of Giant Enterprise, 1860–1914.* By William Graebner 1400
- WALTER GALENSON. *The United Brotherhood of Carpenters: The First Hundred Years.* By Irwin Yellowitz 1400
- SIDNEY L. HARRING. *Policing a Class Society: The Experience of American Cities, 1865–1915.* By James F. Richardson 1401
- ROBERT MIRAK. *Torn Between Two Lands: Armenians in America, 1890 to World War I.* By Richard G. Hovannisian 1401
- DEWEY W. GRANTHAM. *Southern Progressivism: The Reconciliation of Progress and Tradition.* By Lewis L. Gould 1403
- HOWARD JOBLON. *Crossroads of Decision: The State Department and Foreign Policy, 1933–1937.* By Robert D. Schulzinger 1404
- NANCY J. WEISS. *Farewell to the Party of Lincoln: Black Politics in the Age of FDR.* By Harvard Sitkoff 1404
- STEVE NEAL. *Dark Horse: A Biography of Wendell Willkie.* By Bernard F. Donahoe 1405
- WILLIAM T. Y'BLOOD. *Hunter-Killer: U.S. Escort Carriers in the Battle of the Atlantic.* By Clark G. Reynolds 1406
- HAIM GENIZI. *American Apathy: The Plight of Christian Refugees from Nazism.* By Leo P. Ribuffo 1406
- IMANUEL WEXLER. *The Marshall Plan Revisited: The European Recovery Program in Economic Perspective.* By Michael J. Hogan 1407
- DONALD R. RAICHLIE. *New Jersey's Union College: A History, 1933–1983;* ALVIN W. SKARDON. *Steel Valley University: The Origin of Youngstown State.* By H. Warren Button 1408
- MICHAEL W. SEDLAK and HAROLD F. WILLIAMSON. *The Evolution of Management Education: A History of the Northwestern*

University J. L. Kellogg Graduate School of Management, 1908–1983. By G. Wallace Chessman	1409	ENRIQUE D. DUSSEL. <i>Historia general de la Iglesia en America latina</i> . Volume 1, part 1, <i>Introduccion general a la historia de la Iglesia en America latina</i> . By Richard E. Greenleaf	1416
JAMES SLOAN ALLEN. <i>The Romance of Commerce and Culture: Capitalism, Modernism, and the Chicago-Aspen Crusade for Cultural Reform</i> . By Thomas Bender	1409	DAVID CARRASCO. <i>Quetzalcoatl and the Irony of Empire: Myths and Prophecies in the Aztec Tradition</i> . By Edward E. Calnek	1417
H. CRAIG MINER. <i>The Rebirth of the Missouri Pacific, 1956–1983</i> . By John F. Stover	1410	WOODROW BORAH. <i>Justice by Insurance: The General Indian Court of Colonial Mexico and the Legal Aides of the Half-Real</i> . By Murdo J. Macleod	1418
CATHERINE A. BARNES. <i>Journey from Jim Crow: The Desegregation of Southern Transit</i> . By Robert L. Zangrando	1411	EKKEHART KEEDING. <i>Das Zeitalter der Aufklärung in der Provinz Quito</i> . By O. Carlos Stoetzer	1419
RONALD H. SPECTOR. <i>Advice and Support: The Early Years, 1941–1960</i> . By George C. Herring	1411	NICHOLAS P. CUSHNER. <i>Jesuit Ranches and the Agrarian Development of Colonial Argentina, 1650–1767</i> . By James D. Riley	1419
FREDERICK C. MOSHER. <i>A Tale of Two Agencies: A Comparative Analysis of the General Accounting Office and the Office of Management and Budget</i> . By Donald R. Whitnah	1412	DAVID BUSHNELL. <i>Reform and Reaction in the Platine Provinces, 1810–1852</i> . By Richard J. Walter	1420
DAVID W. REINHARD. <i>The Republican Right since 1945</i> . By Robert Griffith	1413	RICHARD W. SLAYTA. <i>Gauchos and the Vanishing Frontier</i> . By William J. Fleming	1421
JONATHAN MARTIN KOLKEY. <i>The New Right, 1960–1968, with Epilogue, 1969–1980</i> . By Thomas C. Reeves	1414	JOHN W. F. DULLES. <i>Brazilian Communism, 1935–1945: Repression during World Upheaval</i> . By Jordan Young	1421
BYRON E. SHAFER. <i>Quiet Revolution: The Struggle for the Democratic Party and the Shaping of Post-Reform Politics</i> . By Jim F. Heath	1414	HORTENSIA PICHARDO. <i>Biografia del Colegio de San Cristóbal de La Habana</i> . By Louis A. Pérez, Jr.	1422
		LOUIS A. PÉREZ, JR. <i>Cuba between Empires, 1878–1902</i> . By Jules R. Benjamin	1423
		WILLIAM W. BOYER. <i>America's Virgin Islands: A History of Human Rights and Wrongs</i> . By Gordon K. Lewis	1423
		Collected Essays	1425
		Documents and Bibliographies	1433
		Other Books Received	1436
		Annual Index	1441
		Index of Advertisers	48(a)

CANADA

DALE GIBSON <i>et al.</i> <i>Attorney for the Frontier: Enos Stutsman</i> . By R. C. Macleod	1415
PATRICIA T. ROOKE and R. L. SCHNELL. <i>Discarding the Asylum: From Child Rescue to the Welfare State in English-Canada (1800–1950)</i> . By Neil Sutherland	1415

LATIN AMERICA

FRANCISCO CUEVAS CANGINO. <i>Bolívar en el tiempo</i> . By Robert H. Davis	1416
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Herbert Baxter Adams and the Study of Local History

JOHN HIGHAM

ON ARRIVING AT JOHNS HOPKINS UNIVERSITY when it opened in the fall of 1876, Herbert Baxter Adams's first task was to find something to do. A dapper young man of boundless ambition, his Heidelberg Ph.D. fresh in hand, Adams had the title of Fellow, a new position in America calling chiefly for the pursuit of research. In recruiting Johns Hopkins's original faculty, President Daniel Coit Gilman had failed to locate a professor of history or of social science. Instruction in history was placed in the charge of a part-time associate, Austin Scott, who commuted from Washington where he was the assistant to the great George Bancroft. Bringing over to Baltimore some books and manuscripts from Bancroft's library, Scott put his students to work on the genesis of the famous Ordinance of 1787. Adams was already formulating for himself a large agenda rooted in his graduate studies in Germany, but he could not resist an opportunity to be useful and to make a place for himself at Hopkins.¹ Plunging into Scott's documents, he focused unerringly on a problem of special interest to Marylanders. The result was a paper entitled "Maryland's Influence in Founding a National Commonwealth," which Adams read to the Maryland Historical Society in 1877 and later described, with characteristic satisfaction, as "the first original work done in the historical department of the Johns Hopkins University."²

Adams had discovered that Maryland's delegates to the Continental Congress framed the first formal proposal empowering Congress to define the western boundaries of the several states. According to the Maryland plan, Congress should erect new independent states in the transmontane region, where the charters of some of the original states created vast, conflicting claims. Adams's paper traced what he called "the sturdy opposition of [Maryland] to the grasping claims of Virginia and the larger States" until, in 1781, Maryland's demands were satisfied sufficiently to permit ratification of the Articles of Confederation. Thus, the young

¹ John Martin Vincent, "Herbert B. Adams," in Howard W. Odum, ed., *American Masters of Social Science* (New York, 1927), 105–07. This remains the best biographical sketch, but also see Hugh Hawkins, *Pioneer: A History of the Johns Hopkins University, 1874–1889* (Ithaca, 1960), 79–90, 171–73, 227.

² Adams, "The Germanic Origin of New England Towns," *Johns Hopkins University Studies in Historical and Political Science* [hereafter, *JHUSHPS*], 1st ser., no. 2 (1882): 38n.

historian proclaimed with italicized emphasis, "*Maryland laid the keystone of the Confederation and . . . of the American Union.*"³

He then drove home this rousing declaration with a lesson from the political science he had learned at Heidelberg. In the creation of a national government, Adams said, the community of material interests that arises from possession of a national domain is more fundamental than written constitutions. "No state without a people," the young historian intoned, "no state without land: these are the fundamental principles of political science and were recognized as early as the days of Aristotle."⁴ Without the common interest that a national domain engendered, the feeble Confederation could not have become a national state. In short, Maryland's land policy established the material basis for an enduring nation. The substantial Baltimoreans who comprised Adams's audience must have smiled and nodded.

Adams's assiduous cultivation of the local history buffs who were already entrenched in Baltimore when he came to town has been forgotten. Adams is remembered only among professional historians, for whom he figures chiefly as the preeminent founder of their guild. When he arrived at Johns Hopkins, the teaching of history as a separate and distinct subject in American colleges and universities was just beginning. The writing of history was in the hands of local antiquarians, journalists, and a few gifted gentlemen like Bancroft and Francis Parkman. Adams took the lead in changing all that. Replacing Scott, he developed a famous seminar in which graduate students were taught a style of scholarship and acquired a corporate identity that separated them decisively from the amateurs who had long dominated historiography in America. Under Adams's inspiration, professional historians seized command of the field.⁵

Such is the conventional wisdom. Although not incorrect, it does not fit well with the tableau I have just sketched. If Adams intended to set out on a new course, why did he first choose to address the principal body of amateur historians in the vicinity and in terms obviously designed to reach their hearts as well as their heads? Why, furthermore, did he indulge in the kind of local boosterism—backed by facile analogies and far-fetched inferences—that was the stock in trade of all historical societies at that time? Why did this fugleman of scientific history start out with a calculated appeal to local pride?

These questions are difficult to answer so long as we insist on forcing Adams into the Procrustean mold of "the professional historian"—a type that did not exist when he defined his own role. So far as I know, Adams never described himself as a

³ Adams, "Maryland's Influence upon Land Cessions to the United States," *JHUSHPS*, 3rd ser., no. 1 (1885): 29, 40. This is a reprint, with a more modest title, of the paper that first appeared in 1877 as a separate publication of the Maryland Historical Society.

⁴ *Ibid.*, 49–51.

⁵ My own account of the formation of the historical profession qualified the conventional disjunction between professional and amateur. But the reasons for the surprising overlap between the two categories in the late nineteenth century were left somewhat uncertain, as Laurence Veysey pointed out. The present essay examines the history Adams himself wrote, and the seminar he conducted. For a more specific explanation of his mediating role, see John Higham *et al.*, *History: Humanistic Scholarship in America* (Englewood Cliffs, N.J., 1965), 8–16; and Veysey, "The Plural Organized Worlds of the Humanities," in Alexandra Oleson and John Voss, eds., *The Organization of Knowledge in Modern America, 1860–1920* (Baltimore, 1979), 99.

professional historian, nor did he urge on others any spirit of professional autonomy. Instead of identifying Adams as the founder of a profession that succeeded, we might better conceive of him as the standard bearer of a cultural enterprise that failed. Much of Adams's energy went to forging a broad alliance between the teachers of history, whom he was training, and the much larger number of local historians, whose support he eagerly solicited and whose status as pillars of the community he yearned to share.

Both intellectual and personal inclinations qualified Adams to play a mediating role. Unlike the strictly academic historians who came after him, Adams never wanted to specialize, compartmentalize, or criticize. His teaching and lecturing ranged by his own choice across the history of ancient, modern, and Oriental civilizations.⁶ His writing and editing aimed to put different things together. His personal style was conciliatory. A zeal to link and consolidate—combining the old with the new, the citizen with the teacher, and local history with the broadest currents of national and international life—explains the history that Adams himself wrote and the organizational initiatives he espoused.

AN UNDERSTANDING OF ADAMS'S DESIGN is essential to a comprehension of his failure. Not enough attention has been paid to the fact that Adams's own publications were quickly superseded and that the institutions he launched either declined or moved out of his control. The story of how all of this happened can throw some light on the split between amateur and professional historians that Adams tried, but failed, to prevent. To discover what went wrong with his organizational strategies, it will be useful to begin with his work as a scholar.

Adams did not follow up his early foray into the history of the public lands, although he encouraged some of his students to do so and thus deserves more credit for Frederick Jackson Turner's discoveries than he has customarily received.⁷ Instead, Adams, as already suggested, conceived another program of research during his first months at Hopkins, if not before, and his first decade in Baltimore was devoted principally to the execution of that program. Its focus was the comparative study of local institutions in early America with the object of demonstrating deep underlying continuities in the character of the American people.

As Adams said later, his interest in the history of local self-government was awakened in one of his Heidelberg seminars by a historiographical controversy over the origins of early medieval towns. Some scholars claimed that the Italian

⁶ W. Stull Holt, ed., *Historical Scholarship in the United States, 1876–1901: As Revealed in the Correspondence of Herbert B. Adams* (JHUSHPs, 56th ser., 1938), 146. On successive weeks Adams presented to his seminar papers on "Oriental Institutions of Learning"—which dealt with Egypt, the Jews, and India—and on "Confucius and Chinese Education." Records of the Historical Seminary, October 17–24, 1890, Milton S. Eisenhower Library, Johns Hopkins University, Baltimore, Md., Herbert Baxter Adams Papers [hereafter, Adams Papers], box 59. That Adams's catholic intellectual interests were not uncommon among the early professors of history has been amply demonstrated by Deborah Haines. See her "Scientific History as a Teaching Method," *Journal of American History*, 63 (1976–77): 892–912.

⁷ Adams's enthusiasm for research in the "vast questions lying back of the disposal and settlement of our Public Lands" is evident in "Maryland's Influence upon Land Cessions," 5–6.

communes sprang from late Roman villas. Others, the Teutonists, argued that the liberties of the medieval town were derived from the ancient customs of Teutonic villages as described by Tacitus. According to this theory, the Germanic villagers regulated their own affairs through local assemblies. These communal, self-governing institutions antedated the feudal system and, here and there, survived its oppressive weight. Modern democracy, therefore, originated in the forests of Germany. Adams joined the Teutonists.⁸

Adams found the theory compelling not only because of the formidable scholarly authority of his German professors but also because of its adoption by leading English historians of the third quarter of the nineteenth century. John Kemble's influential book on Anglo-Saxon colonization in England depicted the Saxons as bringing with them from Germany the self-governing village community. Bishop Stubbs elaborated on the survival of local liberties beneath the feudal pyramid imposed by the Norman Conquest—liberties imbedded in such institutions as courts of the shire and the hundred. John Russell Green in 1874 spread these ideas before a very large American public. The opening pages of his *Short History of the English People*, the historical best seller of the Gilded Age, invested the Teutonist thesis with an unforgettable piety. "It is with a reverence such as is stirred by the sight of the head-waters of some mighty river that one looks back to these tiny moots, where the men of the village met to order the village life and the village industry, as their descendants, the men of a later England, meet in Parliament at Westminster, to frame laws and do justice for the great empire which has sprung from this little body of farmer-commonwealths."⁹

Both the English and the German Teutonists were engaged in defining their own national characters in terms of age-old customs organically rooted in local institutions and only gradually nationalized. The English historians gave this basically romantic theory a strongly libertarian inflection, which explains much of its appeal to upper-class Americans in the Gilded Age. Transposed to the United States, the Teutonist thesis said that American democracy was not a product of revolution, nor was it truly formulated in abstract doctrines concerning the rights of man. Instead, it was the distinctive ethnic heritage of a people who had learned self-government by running their own affairs and defending local liberties against centralized power.

Adams's contribution was simply to move the area of scholarly inquiry from the beginning of the Middle Ages to the beginning of American history. What Kemble and his successors had found in the early Saxon settlements in England, Adams set out to find in the early English settlements in North America. He went about the task with zeal and enthusiasm. His object was not to test a theory but to extend it, so, of course, he found what he was looking for.

Himself a New Englander of obscure but ancient lineage, Adams concentrated his own research on the towns of eastern Massachusetts. Teaching during the spring term at Smith College for several years enabled him to spend many months

⁸ Raymond J. Cunningham, "The German Historical World of Herbert Baxter Adams, 1874–1876," *Journal of American History*, 68 (1981–82): 269–70.

⁹ Green, as quoted in J. W. Burrow, *A Liberal Descent: Victorian Historians and the English Past* (Cambridge, 1981), 125.

in local archives. Adams felt confident, he told a friend in 1880, that his study of Plymouth “will be for town history what Huxley’s work on the crawfish is for biology, i.e., typical of many things besides those described.”¹⁰ Adams never produced a major study of an individual town, nor did he write a solid book on any subject. But in 1882–83 he was able to publish in the first volume of the *Johns Hopkins Studies in Historical and Political Science* four essays on the survival and revival of early English institutions in the towns of New England. These essays, which he had already read before various local historical societies, were entitled “The Germanic Origin of New England Towns,” “Saxon Tithing-Men in America,” “Norman Constables in America,” and “Village Communities of Cape Anne and Salem.” All argued that ancient customs, long submerged in modern England, had reappeared on this side of the Atlantic in such practices as communal landholding, town meetings, the appointment of selectmen, and the building of stockades.

Today these essays by Adams seem as shallow as they are pretentious. He was not a first-class historical scholar, partly because he cared only about continuity and took no interest whatever in change and partly because he ignored any construction of evidence other than what his theory dictated. He never admitted, for example, that early colonial settlers had a more immediate reason for putting up palisades than the mysterious compulsion of their Saxon ancestry. Yet Adams’s work was important, because it laid out an agenda for research that was intended to engage both the graduate students he trained and the local amateur historians with whom he constantly hobnobbed.

To the students Adams offered an introduction to historical research that would begin on their own home ground. On taking full charge of the Johns Hopkins Seminary in Historical and Political Science in the fall of 1881, Adams required that each graduate student undertake an original investigation into the institutional history of his or her own locality. These studies, Adams advised, should be broadly conceived to show how local phenomena provide a foundation for national and international life, but “the first step in History is to know thoroughly the district where we live.”¹¹ Since Hopkins attracted students from all over the country, Adams was able to include in the first volume of the *Johns Hopkins Studies* papers on local government in Illinois, Michigan, Pennsylvania, and South Carolina, on parish institutions in Maryland, on old Maryland manors, and on the evolution of Connecticut from separate townships. Fortunately, Adams did not insist that his students specifically address the Teutonic thesis, but the spirit of their research was wholly consistent with it.

To the many local historical societies that proliferated in the post-Civil War decades, Adams proposed an agenda based squarely on what they were already doing.¹² The societies were dedicated to civic improvement under the leadership of

¹⁰ Adams, as quoted in David D. Van Tassel, “From Learned Society to Professional Organization: The American Historical Association, 1884–1900,” *AHR*, 89 (1984): 947.

¹¹ Adams, “Co-operation in University Work,” *JHUSHPS*, 1st ser., no. 2 (1882): 48. Adams included a detailed account of the early years of the Hopkins seminar in *The Study of History in American Colleges and Universities* (United States Bureau of Education, Circular No. 2, 1887), 171–79.

¹² David D. Van Tassel, *Recording America’s Past: An Interpretation of the Development of Historical Studies in America, 1607–1884* (Chicago, 1960), 185–89. On the rise and character of the societies, also see George H.

a cultural elite and to the celebration of deeds and values the local communities cherished. Adams told the societies, in effect, that stories of individual achievement were necessarily episodic. To appreciate fully the continuity of past and present, local historians should study the origins of local institutions. The vast majority of available local histories, he declared in an early public address, ignore the most important question they should address: the genesis of the town as an institution.¹³ To underline the significance of institutional origins, Adams brought one of England's foremost Teutonists, E. A. Freeman, to Baltimore in 1881 to lecture at the Peabody Institute. Freeman provided Adams with a prestigious endorsement of the notion that local institutions transmit the seeds of national character. The English migration to America in the seventeenth century, Freeman argued, revived local institutions that Anglo-Saxons had relied on in their migration to England in the fifth and sixth centuries. Consequently, an English heritage still remained in America "the kernel round which everything grows and to which everything else assimilates itself."¹⁴

While offering the conceptual stimulus of a wide-ranging, emotionally appealing theory, the Adams agenda also promised local historical societies a new infusion of energy. Keenly aware of the practical benefits to be gained through "associations of men and money" (as he confessed to Gilman),¹⁵ Adams urged his students to be active in local organizations.

Local studies should always be connected in some way with the life of the community and should always be used to quicken that life to higher consciousness. A student, a teacher, who prepares a paper on local history or some social question, should read it before the Village Lyceum or some literary club or an association of teachers. If encouraged to believe his work of any general interest or permanent value, he should print it in the local paper or in a local magazine. . . . It is highly desirable that every paper which appears in connection with the Johns Hopkins University Studies should bear the stamp of corporate recognition by some worthy local organization. Such approval, and especially such preliminary publication, will introduce an unknown student to science with credentials from a local constituency.¹⁶

Thus, while Adams's historiographical program envisaged an eventual rewriting of national history in terms of local history, his organizational leadership pointed in exactly the same direction: to wit, a working partnership between the best of the local historians and the nationally oriented academics.

To understand the strength of this commitment, it is helpful to appreciate the common class standing that facilitated the partnership. Although most university professors, like Adams himself, came from relatively modest social backgrounds, they were routinely received into the social elite in the communities where they

Callcott, *History in the United States, 1800–1860: Its Practice and Purpose* (Baltimore, 1970), 35–45; Joseph W. Cox, "The Origins of the Maryland Historical Society: A Case Study in Cultural Philanthropy," *Maryland Historical Magazine*, 74 (1979): 103–16; and Frederic Cople Jaher, *The Urban Establishment: Upper Strata in Boston, New York, Charleston, Chicago, and Los Angeles* (Urbana, Ill., 1982).

¹³ William E. Foster to Herbert Baxter Adams, August 29, 1881, Adams Papers, box 33.

¹⁴ Edward A. Freeman, "An Introduction to American Institutional History," *JHUSHPS*, 1st ser., no. 1 (1882): 31.

¹⁵ Holt, *Historical Scholarship in the United States*, 5. This entire letter describes Adams's relations with local and state historical societies in illuminating detail.

¹⁶ Adams, "Co-operation in University Work," 51.

taught. Professors shared with the urban patrician class a common style of dignified dress, a common standard of gentlemanly behavior, a common Anglophile outlook, a common acceptance of an elaborate social hierarchy, and a common zeal to elevate and civilize a rough-hewn, overly materialistic people.¹⁷ In mid-nineteenth-century America historical societies were foci of this kind of cultural leadership.

HAPPY TO BE A PART of so congenial a social milieu and supremely gifted in tact and diplomacy, Adams labored tirelessly to promote cooperation between the local cultural establishment and the emerging professoriat. His next initiative, after demonstrating his talents to the Maryland Historical Society, came as secretary of a historical and political science association that President Gilman launched in 1877.¹⁸ This was intended as a forum for scholarly discourse open to a cultivated public. Taking advantage of the proximity of Washington, Adams frequently invited distinguished outsiders to address the association on matters of general cultural import. On replacing Scott as director of the seminar in history and political science, he continued the same practice. The association and the seminar coalesced. Thus, the famous Johns Hopkins seminar, in which aspiring college teachers were trained to search for new truth, also functioned as a place where nonacademic pundits could present their own investigations or simply talk about contemporary public issues.

Simultaneously Adams worked closely with the Maryland Historical Society and the Peabody Institute to quicken the study of Maryland history. As early as 1878 Adams was talking with "certain Baltimore gentlemen" about ways and means of persuading the state to publish some of its colonial records.¹⁹ The Maryland Historical Society took up the issue. When Freeman came to lecture at Peabody, Adams extracted from him an open letter extolling the value of the unpublished archives in Annapolis and urging the Maryland legislature to underwrite their systematic publication. A letter-writing campaign in the Baltimore newspapers followed. In 1882 the state authorized just what Adams wanted: the transfer of the mass of colonial and Revolutionary archives from Annapolis to Baltimore and a subvention to the Maryland Historical Society for publication of these early records.²⁰ Largely through Adams's initiative and political skill, the historical society took a major step in the advancement of historical research, and the academic community at Hopkins gained immediate access to a magnificent corpus of sources for Maryland history.

Adams's best-known accomplishment as an entrepreneur of scholarship was the creation just one hundred years ago of the American Historical Association. This is often supposed to have been from the outset a determinedly professional body,

¹⁷ Higham *et al.*, *History: Humanistic Scholarship in America*, 8–11. For perhaps the best statement of this common value system, see James Russell Lowell, "Harvard Anniversary Address," 1886, in *The Writings of James Russell Lowell*, 6 (Cambridge, Mass., 1890): 137–80.

¹⁸ Records of the Historical and Political Science Association, Adams Papers, box 58. Also see Hawkins, *Pioneer*, 113–15.

¹⁹ Records of the Historical and Political Science Association, March 29, 1878, Adams Papers, box 58.

²⁰ Adams, "Mr. Freeman's Visit to Baltimore," *JHUSHPS*, 1st ser., no. 1 (1882): 9–10.

aloof from the amateurs engaged in local, antiquarian work. But nothing was further from Adams's intentions. He ran the organization almost single-handedly for about a decade, and throughout that time he strove unremittingly to include and recognize the gentleman-historians and to serve and guide the local historical societies.²¹

The initial proposal for a national organization to promote systematic training and study in history came not from the Hopkins professor but from his namesake Charles Kendall Adams, the new president of Cornell University. It was H. B. Adams, however, who made the suggestion a reality. Because he sent out the call for an organizational meeting, he decided who the founders should be and served from the outset as secretary. Significantly, the first meeting consisted mostly of nonacademic historians, though the inner circle—a committee of five who drew up the constitution—was predominantly professional. The implicit theory of the association was that the professors would lead and yet welcome and honor outstanding amateur historians and seek to coordinate efforts of the many local historical societies.

It was not easy for Adams to balance the claims of professors and amateurs. After the annual meeting of 1888 a young professor complained, "There were more nobs than usual in attendance. . . . I am a little inclined to think the thing is getting into the hands of elderly swells who dabble in history, whereas at first it was run by young teachers, which I think made it more interesting."²² On the other hand, Edward Eggleston, an outstanding amateur historian, complained a year later that the American Historical Association "seems to be run in the interest of college professors only and to give those of us who are not of that clan the cold shoulder."²³ Still, there was a balance of sorts until 1895, when a group of university professors rebelled against Adams's leadership and turned the association in a more exclusively professional direction. They did so in two ways: first, by shifting the annual meeting from Washington, D.C., where amateur historians predominated, to various university campuses; second, by creating a new scholarly journal—the *American Historical Review*—that was placed under strictly professional editorship and removed from Adams's influence.

What occasioned this rebellion and the subsequent separation between the worlds of the amateur and the professional historian? It is hard not to believe that the separation was in large measure unavoidable. The young teachers of history—trained by Adams and others at the leading universities to search for undiscovered truth—could not identify themselves wholeheartedly with a local civic culture, as the physicians and lawyers of an earlier era had done. The academic historians were caught up in a new, disciplinary professionalism that invaded all fields of knowledge.²⁴ The new pattern transferred intellectual authority to clusters of

²¹ Here I have followed, especially, Van Tassel's detailed account in "From Learned Society to Professional Organization."

²² Jameson to John Jameson, January 5, 1889, printed in Elizabeth Donnan and Leo F. Stock, eds., *An Historian's World: Selections from the Correspondence of John Franklin Jameson* (Philadelphia, 1956), 46–47.

²³ Eggleston, as quoted in David D. Van Tassel, "The American Historical Association and the South, 1884–1913," *Journal of Southern History*, 23 (1957): 468.

²⁴ For Thomas Bender's interpretation of this transition, see "The Cultures of Intellectual Life: The City and

specialists, who fiercely insisted on autonomy in setting standards and validating results without regard for particular interests or local needs. Moreover, the professional communities did their best work by testing and elaborating recondite theories. Thus, the interpretations produced by professional historians assigned causal importance to processes more or less remote from the immediate experience that amateur scholars still found self-explanatory. The mix of local dignitaries and academic specialists that Adams tried to maintain was bound to break down.

Yet it was perhaps not inevitable that the separation between amateur and professional became, in the study of history, a yawning chasm. History is the least arcane of all sciences. It is preeminently the discipline in which dialogue between amateur and professional might have remained open and fruitful without compromising the distinctive purposes of either group. That was certainly Adams's intent, and he was adroit in pursuing it. Why did he fail?

One possible explanation may be the weakening of local ties and identities throughout much of American middle-class life in the first half of the twentieth century. The local community constituted the common ground on which academic and amateur scholars met. A shared affection for it was essential to their partnership. But the attachment of the young professional historian to a particular locality suffered as his reputation and advancement came to depend on a willingness to move to another university in another part of the country. The mobile professor necessarily invested his energies in the accumulation of an intellectual capital as portable as his family and personal possessions. Local history was literally left behind.

Similar pressures probably deprived local historical societies of nonacademic talent as well. Like the new disciplinary professions, the older gentlemanly vocations were drawn into national networks that reduced the importance of strictly local relationships. In the nineteenth century men at the top of the occupational structure in American cities were less likely than lower-status workers to move elsewhere. Their local interests were extensive, their ties with one another dense and varied. In the twentieth century the affluent strata of urban society have often become the most transient, and many of those who remain settled in one locality withdraw from active participation in local institutions as their careers and horizons move outward to wider but also more specialized connections.²⁵ It is reasonable to suppose, therefore, that local history has actively engaged a smaller proportion of the talent and cultural leadership in many cities than it did in an earlier era.

Thus, I believe that the high level of geographical mobility and the accompanying development of translocal segmentation, which have characterized American

the Professions," in John Higham and Paul K. Conkin, eds., *New Directions in American Intellectual History* (Baltimore, 1979), 181–95. For a later amplification, see Bender's "The Erosion of Public Culture: Cities, Discourses, and Disciplines," in Thomas L. Haskell, ed., *The Authority of Experts: Studies in History and Theory* (Bloomington, Ind., 1984), 84–106.

²⁵ Stephan Thernstrom, *The Other Bostonians: Poverty and Progress in the American Metropolis, 1880–1970* (Cambridge, Mass., 1973), 228–30; and Donald A. Clelland and William H. Form, "Economic Dominants and Community Power: A Comparative Analysis," *American Journal of Sociology*, 69 (1963–64): 511–21. The persistence of a stable leadership in the midst of flux in the nineteenth century is explored in Richard S. Alcorn, "Leadership and Stability in Mid-Nineteenth Century America: A Case Study of an Illinois Town," *Journal of American History*, 61 (1974–75): 685–702.

middle-class life through most of the twentieth century, have been underlying reasons for a decline in the study of local history. But sociological interpretations never fully excuse us from looking at the imponderables of leadership in human affairs. The inducements of mobility within a national academic system hardly explain the suddenness with which Adams's plans for the promotion of local history collapsed. Adams was manifestly unable to resist effectively the abandonment of his program by his best students. His program failed, quite simply, from a lack of intellectual substance.

THE FIRST IMPLICIT CHALLENGES to the kind of local history Adams laid out as a model in 1881–82 came within two or three years from two of his most remarkable students. Woodrow Wilson arrived in Baltimore in the fall of 1883, the goal of his graduate training already firmly in mind. He wanted to continue the studies in comparative politics that he had begun as an undergraduate at Princeton, under the influence of Walter Bagehot, and had pursued independently for several years. His object was to assess the working of representative government in the United States, Britain, and elsewhere, with particular attention to the shortcomings of the American model. Dismayed to learn that all students were expected to dig “in the dusty records of old settlements and colonial cities” for the purpose of “rehabilitating in authentic form the stories . . . of the first colonists,” Wilson pleaded with Adams to be allowed to follow his own bent. Adams not only assented but told the startled and elated young man that the project he proposed was just what Adams wanted to see done.²⁶ Before the academic year ended Wilson read to Adams's seminar two stunning chapters from his forthcoming book, *Congressional Government* (1885). Wilson's approach to comparative problems diverged significantly from that of Adams, partly because Wilson focused on national rather than local government but even more because Wilson concentrated on differences between the United States and Britain rather than on similarities and continuities. Nevertheless, after Wilson's first seminar report Adams grandly commented that Wilson's studies of the national government were a valuable addition, “as this subject has long been intended to be a part of the work on institutions undertaken in this department.”²⁷

Wilson's success may have encouraged another seminar member, John Franklin Jameson, to strike out on his own. Three years younger than Wilson, Jameson was less self-assured; he was also more dependent on Adams. Having submitted just the kind of dissertation Adams desired—a study of an early Long Island town—he had received his Ph.D. in 1882 and was then kept on temporarily as an associate while waiting for a suitable position to open up elsewhere. In the style of the German universities on which Hopkins was modeled, he was Adams's assistant. Jameson fumed in private at the frequent tedium of the Adams seminar and the lack of

²⁶ Wilson to Ellen Louise Axson, October 16, 1883, in Arthur S. Link, ed., *The Papers of Woodrow Wilson*, 2 (Princeton, 1967): 479–80. Also see Henry W. Bragdon, *Woodrow Wilson: The Academic Years* (Cambridge, Mass., 1967), 57–64, 75–80, 95–113.

²⁷ Records of the Historical Seminary, May 8, 1884, Adams Papers, box 58.

sharp criticism; he grumbled when students obediently traced institutions “back nearly to when our ancestors chattered in the tree-tops.”²⁸ Nevertheless, he thought it expedient at first to accept an assignment from Adams to investigate parallels between the village communities of the Teutonic race and those of the ancient Greeks.²⁹ After Wilson’s dramatic assertion of independence, Jameson staked out a new area of historical research that would free him from discipleship and implicitly challenge Adams’s Teutonism. In January 1885 the young associate read to Adams’s seminar a series of papers arguing that a longstanding preoccupation with the colonial period, with local history, and with the more colorful episodes of national history had left the history of the states as the most neglected level of American history. State politics, Jameson contended, had given national politics their shape and direction, and local records should be used to illuminate these broader developments.³⁰

Once again Adams imperturbably embraced the incipient rebellion. Addressing the seminar after Jameson delivered his second paper, Adams urged its members to take up the line of research his subaltern was recommending, since it would be an excellent means of demonstrating connections between Europe and America.³¹ That was one of the last things Jameson had in mind. He was shifting attention away from remote European antecedents to the immediate origins of the new governments created during the American Revolution.³² Adams’s benevolent and irenic comment served, however, to bridge the gap between his own agenda and Jameson’s, one rooted in the Old World, the other in the New.

Henceforth, it must have been clear that Adams would not actively defend the Teutonist thesis against rival approaches; instead, he would confer his approval on whatever new initiatives his students might take. After 1887 the number of contributions to American local history in the *Johns Hopkins Studies* markedly declined.³³

The finest scholar of early American history who emerged from Adams’s seminar was Charles McLean Andrews. No scoffer like Jameson, Andrews was fascinated by the Teutonist thesis but highly skeptical of the evidence for it. His doctoral dissertation examined minutely the founding of three Connecticut towns,

²⁸ Jameson to John Jameson, February 13, 1882, printed in Donnan and Stock, *An Historian’s World*, 21. Also see *ibid.*, 22, 25 n., 26, 32.

²⁹ Freeman, “Introduction,” *JHUSHPS*, 1st ser., no. 1 (1882): 14 n.; and Donnan and Stock, *An Historian’s World*, 25 n.

³⁰ Jameson, “An Introduction to the Study of the Constitutional and Political History of the States,” *JHUSHPS*, 4th ser., no. 5 (1886): 5–29.

³¹ Records of the Historical Seminary, January 23, 1885, Adams Papers, box 58.

³² Jameson’s “Introduction” was not without influence in altering the tenor of the Adams seminar at Hopkins. Four years later Jameson was able to publish a volume of essays by Hopkins students, *Essays in the Constitutional History of the United States in the Formative Period, 1775–1789* (Boston, 1889). About the same time a similar paper by Albion Small, “Relations of the Continental Congress to the Colonies,” happened to be presented to the seminar on an evening when Woodrow Wilson, then a visiting lecturer, attended. Wilson took the occasion to praise Small and to “deprecate . . . the study of our institutions from the European standpoint.” Adams was absent that evening. One wonders if Wilson would have been less outspoken in his presence. Records of the Historical Seminary, March 8, 1889, Adams Papers, box 59. On the planning of Jameson’s book, see Donnan and Stock, *An Historian’s World*, 42.

³³ Monographs dealing with particular localities were the predominant type only in the first four volumes of the *Johns Hopkins Studies* (1883–86). State and national studies predominated through the late 1880s and 1890s. Local history, feebly represented in the 1890s, thereafter disappeared.

concluding that they were not (as Teutonists supposed) the institutional “seeds” from which the commonwealth of Connecticut had sprouted.³⁴ Andrews then immediately moved back to the “village community” of the Anglo-Saxons, plunging fearlessly into a thicket of Anglo-Saxon and Latin sources. A modulated but decisive paper that he presented to Adams’s seminar in 1890, a year after he had begun teaching at Bryn Mawr, showed that the very existence of self-governing communities of independent freemen, standing at the dawn of English and German history, was undemonstrable, improbable, and intellectually out of date.³⁵

Andrews did not write as a lonely iconoclast. He drew on, and identified himself with, the best current scholarship in England, where opponents of the Teutonist thesis were rapidly gaining ground. Moreover, Andrews aimed his critique not at Adams but at the older generation of English historians, especially Freeman; he avoided all mention of the grandiloquent Teutonist essays his mentor had published just seven years before. Adams returned the kindness by urging his former student to expand the paper into a book and offering to publish it. Andrews eagerly complied. When *The Old English Manor* appeared as an extra volume in the *Johns Hopkins Studies* in 1892, Andrews wrote dutifully to his former professor, “I trust that you are satisfied with the work. . . . I feel sure it cannot be accused of being hastily put together or being based on second hand authorities.”³⁶

Andrews’s expert demolition shattered whatever intellectual authority the Teutonist thesis still possessed. By doing so he deprived local history of its special significance as the source and seat of American freedom. Adams had mortgaged the study of local history to a flimsy, outmoded theory—a theory, moreover, that Adams had neither the stomach to defend nor the wit to revise. Is it any wonder that the search for significant themes in American history turned almost exclusively to the state, the national, and—among colonial historians—the imperial levels?

FOR ADAMS THE QUEST FOR GRAND THEMES in history was over. In the late 1880s and after, his own writing shifted principally to current problems in education and social reform. For the U.S. Bureau of Education he produced a number of studies of higher education in various states. In other publications he called attention to

³⁴ Andrews, “The River Towns of Connecticut: A Study of Wethersfield, Hartford, and Windsor,” *JHUSHPS*, 7th ser., nos. 7–9 (1889): 8–9. Here Andrews’s very limited and specific qualification of Teutonist thought was aimed at Alexander Johnston’s essay, “The Genesis of a New England State,” in the first volume of the *Johns Hopkins Studies*. For evidence of Andrews’s filial attachment to Adams, see A. S. Eisenstadt, *Charles McLean Andrews: A Study in American Historical Writing* (New York, 1956), 9–10.

³⁵ Records of the Historical Seminary, December 5, 1890, Adams Papers, box 59. Andrews presented the same or a closely related paper to the American Historical Association later the same month and incorporated it into the introduction to *The Old English Manor: A Study in English Economic History* (Baltimore, 1892). See “The Theory of the Village Community,” *Papers of the American Historical Association*, 5 (1891): 47–60. The Teutonist thesis was out of date, according to Andrews, because it rested on a romantic, idealized image of primitive man. This “liberal optimism,” Andrews pointed out, had declined since 1870 as the world learned that humanity is “pervaded with brute instincts” and that “primitive man, whether he were Saxon, Teuton or Aryan, was very much lower down in the scale of human development than the older view was willing to place him.” *Old English Manor*, 4. For an intriguing though complex interpretation of the difference between Adams and Andrews, see Dorothy Ross, “Historical Consciousness in Nineteenth-Century America,” *AHR*, 89 (1984): 927–28.

³⁶ Andrews to Adams, October 23, 1891, May 19, 1892, Adams Papers, box 33. The Teutonist theme in English historiography is treated in J. W. Burrow, “‘The Village Community’ and the Uses of History in Late

many English initiatives in philanthropy and social betterment, encouraging his students especially to admire the historian *engagé* Arnold Toynbee, who founded the settlement movement by inducing Oxford graduates to bring “learning and civilization” to “a savage district” in East London.³⁷ One senses that Adams’s increasing writing and public lecturing in behalf of genteel reform provided a new outlet for his enduring identification with a responsible civic elite when the promise of local history faded. Presciently he understood that the civic involvement his students and younger colleagues were giving up as scholars could be regained in some measure through reform.

By the end of the nineteenth century very few of the younger professional historians were taking an interest in local history or in the state and local societies that fostered it.³⁸ The societies for the most part were content to go their own way. Wisconsin was a notable exception. The State Historical Society of Wisconsin under Reuben Gold Thwaites developed a close, fruitful relation with the University of Wisconsin while also serving all of the historical interests of the people at large. Wisconsin’s example gradually encouraged similar forms of cooperation between amateur and professional in other Midwestern states.³⁹ In the East, however, relations between the two groups may be judged from the content of a paper Jameson gave at the AHA meeting in 1897. He scolded the state and local societies for making much slighter contributions to knowledge than similar bodies in France and Germany, for having improved their publications only marginally in the past forty years, for ignoring everything that had happened since the American Revolution, and for “gross misuse” of their library funds to feed a ravenous interest in genealogy.⁴⁰

It is unlikely that many local historians were on hand to hear Jameson’s attack. The new policies inaugurated in 1895 had produced a dramatic increase in the number of academic members of the AHA and in the range of its academic services. The organization now belonged visibly and overwhelmingly to the professors. Adams acquiesced with his usual good nature in his loss of control over the association.⁴¹ In 1900 he quietly resigned as secretary. Leadership in the historical guild passed into the hands of Jameson, who was the very incarnation of professionalism. To Jameson, for example, the “scientific society” was the true bastion of scholarship because it was “composed of specialists alone and working in unhampered devotion to intellectual ends,” whereas the American university was

Nineteenth Century England,” in Neil McKendrick, ed., *Historical Perspectives: Studies in English Thought and Society in Honour of J. H. Plumb* (London, 1974), 255–84.

³⁷ Adams, “Notes on the Literature of Charities,” in Charity Organization Society of Baltimore, *Report of a Conference on Charities* (Baltimore, 1887), appendix, and pp. 19–23. In 1889 the *Johns Hopkins Studies*, seventeenth series, led off with three inspirational articles about Arnold Toynbee, the settlement movement, and its recent introduction to the United States.

³⁸ J. Franklin Jameson, “The Functions of State and Local Historical Societies with Respect to Research and Publication,” *Annual Report of the American Historical Association, 1897* (Washington, 1898), 56.

³⁹ H. Hale Bellot, *American History and American Historians* (Norman, Okla., 1952), 26–35.

⁴⁰ *Annual Report of the American Historical Association, 1897*, 54–59.

⁴¹ While Jameson was excoriating the local historical societies and their members, Adams was waving an olive branch. “Peace and harmony now reign throughout the American Historical Association,” Adams wrote in his report of the annual meeting. “All recognize the wisdom of meeting in the West as well as in the East, with perhaps a triennial round-up in Washington.” *Annual Report of the American Historical Association, 1897*, 9.

"a body of specialists ruled over by a body of 'prominent citizens.'"⁴² As editor of the *American Historical Review* for more than a quarter of a century, Jameson vigilantly guarded its pages against the antiquarianism he thought almost inseparable from local history.⁴³

Could a different approach to local history at the outset have mitigated this rupture? The question admits of no conclusive response, but it prompts a second question that suggests a feasible alternative. What kind of local history could have engaged amateur historians effectively and some professionals as well? The answer must surely be a narrative history of individuals and human endeavors rather than an impersonal history of institutions. A feeling for a specific theater of action and for the events it dramatizes has been the attraction and strength of amateur history from the time of Homer to the present. In promoting an institutional history that rested on arcane knowledge, a remote past, and fanciful theories, Herbert Baxter Adams offered to amateur historians a scheme of cooperation into which they could not fit.

Perhaps another kind of theory, more tangibly imbedded in the circumstances of American life, could have drawn the active support of amateur scholars. Frederick Jackson Turner's frontier thesis may have accounted in part for the better rapport between academic and nonacademic historians in the Midwest. By focusing on the impact of the local environment, Turner confined causation to a sphere that local historians could comprehend. Teutonism offered them no comparable challenge. It moved the locus of causation out of a familiar habitat, back into an alien and uncertain past where no level-headed empiric wanted to follow. Some of the best of the local historians regarded Adams's Teutonism as symptomatic of the impractical theorizing to which academic scholars were prone. On returning from an early meeting of the American Historical Association, Mellen Chamberlain reported to the Massachusetts Historical Society that the "New Historical School" (which he identified with Freeman and Adams) consisted of "young men mainly of scholastic training, unacquainted with affairs and without opportunities for observing how the elementary facts which make history are colored and even transformed in legislative assemblies, by judicial decisions, and in the tumultuous proceedings of the crowd."⁴⁴ Chamberlain and the other speakers who responded to his paper agreed that the New England town meeting was not a primordial form but a modern, indigenous invention.

So the amateur and professional historians went their separate ways until, in recent years, many of the latter have begun to turn back to the local community. They have done so because the historical profession no longer tempts them away. Opportunities for moving onward and upward, from one locality to another, have sadly contracted. The intellectual appeal of national history has correspondingly waned. Once more the manageable scale and the immediate presence of a concrete

⁴² J. Franklin Jameson, "The American Historical Association, 1884-1909," *AHR*, 15 (1909-10): 2.

⁴³ See Morey Rothberg, "'To Set a Standard of Workmanship and Compel Men to Conform to It': John Franklin Jameson as Editor of the *American Historical Review*," *AHR*, 89 (1984): 957-75.

⁴⁴ *Proceedings of the Massachusetts Historical Society*, 2nd ser., 5 (1890): 265. For similar comments by Abner C. Goodell, Jr., see *ibid.*, 323-24; by Charles Francis Adams, Jr., see *ibid.*, 7 (1892): 207. I am indebted to Raymond Cunningham for bringing to my attention this extended discussion of the origins of the town meeting.

community is both intellectually attractive and emotionally engaging to many young historians. The “new” social history has revived the old promise of institutional history: the promise that community studies can illuminate the general contours of the past.

Revived, but not yet redeemed. Professional historians still bring to community studies a panoply of theories, and the theories still leave the general contours of the past uncertain. Amateur historians probably find the new concepts no more useful than were the doctrines of the Teutonist thesis. The state and local historical societies are now infiltrated by professional scholars who often manage their publication programs, but communication between amateur and professional remains difficult and tenuous. Compelling narrative history is still in short supply. It receives little encouragement either from the seminars of professionals or from the historical societies. Nevertheless, the wheel has come full circle. It is time once more to wrestle with problems that Herbert Baxter Adams failed to solve.

The Gestalt Psychologists in Behaviorist America

MICHAEL M. SOKAL

THE TRANSMISSION AND DIFFUSION OF CULTURES—from the spread of Hellenism after the death of Alexander to the intellectual diaspora that accompanied Hitler's rise—has long interested historians. One subject of recent interest has been the influence of central European emigres on American scientific, intellectual, and artistic life in the early twentieth century.¹ That they enriched American culture in many fields is beyond question, but this transmission was not a simple transplantation to a new environment. The indigenous culture of America has always reacted to and reshaped the impulses it receives from abroad. Furthermore, Americans had sought cultural insights in Germany for more than a century before the Nazis took power.² Before 1914 American students and scholars in many fields went there to acquire the specialized knowledge and methods of their disciplines, but in the Weimar and Nazi eras the migration was in the opposite direction. The travelers and emigres of the later era brought with them a cultural baggage that usually included more than what interested their hosts. What ensued was an interaction of both personalities and ideas, whose import will long be felt in American culture and whose complexity will always challenge the descriptive and

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¹ Donald Fleming and Bernard Bailyn, eds., *The Intellectual Migration: Europe and America, 1930–1960* (Cambridge, Mass., 1969); and Jarrell C. Jackman and Carla M. Borden, eds., *The Muses Flee Hitler: Cultural Transfer and Adaptation in the United States, 1930–1945* (Washington, 1983).

² Carl C. Diehl, *Americans and German Scholarship, 1770–1870* (New Haven, 1978); and Michael M. Sokal, ed., *An Education in Psychology: James McKeen Cattell's Journal and Letters from Germany and England, 1880–1888* (Cambridge, Mass., 1981).

analytic powers of historians. This essay is a case study in one aspect of this cultural diffusion: the transmission of Gestalt psychology from Germany to the United States in the 1920s and early 1930s.

Unfortunately, no one writing on this topic can assume that even a literate audience is familiar with the psychology of the period, especially since it bears little resemblance to the current discipline. In contemporary society psychology is popularly viewed as a clinical profession, practicing not science but therapy. Often it is confused with psychiatry, the medical specialty concerned with abnormal behavior and mental illness. Despite the efforts of many academic psychologists, psychology is today more closely identified with its medical counterpart than, for example, human physiology is with internal medicine. But this was not always true.

Before World War II, American psychology was primarily an academically based science, committed to a greater understanding of human behavior (or the human mind), usually without an explicit concern for the treatment of behavioral (or mental) disorders. To be sure, some psychologists in both Europe and America studied the work of Freud and his followers, and as early as the 1890s some called themselves clinical psychologists.³ But psychology in the 1920s and 1930s concentrated primarily on questions of human (and animal) life—the operation of the senses, the nature of perception, the way in which we learn, and the processes of thought.⁴ Although still being studied today, of course, the old subjects have ceased to dominate psychology. The therapeutic profession has all but swamped the science. Psychology focused on these scientific questions, and experimental psychologists held the positions of highest status in the field. But after 1945 the demands of the returning soldiers led to a reorientation of the discipline.

WITHIN THE EARLIER SCIENCE OF PSYCHOLOGY, there was little agreement on basic principles and approaches. By 1910, at least in America, such schools as structuralism, functionalism, and dynamic psychology could be identified. Historians of psychology have laid too much stress on the differences between the schools and have painted them almost as warring camps, forgetting that all of those involved were primarily psychologists and only secondarily structuralists or functionalists or whatever. Both the diversity and eclecticism of American psychology in this period eased the later reception of Gestalt psychology. By 1919, as the effects of World War I faded, most American psychologists settled down to work within a vaguely defined, functionally oriented behaviorism, which was concerned with the way humans (and animals) actually live and function in the world. Most (but not all) American psychologists shied away from the extreme rhetorical pronouncements of John B. Watson, the self-proclaimed founder of behaviorism, who argued that

³ Clinical psychology before World War II was concerned much more with the diagnosis and evaluation of sensory disabilities and learning problems than with the psychotherapeutic treatment of neurosis. See, for example, Virginia Staudt Sexton, "Clinical Psychology: An Historical Survey," *Genetic Psychology Monographs*, 72 (1965): 401–34; and Robert I. Watson, "A Brief History of Clinical Psychology," *Psychological Bulletin*, 50 (1953): 321–46.

⁴ For an excellent survey of American psychology during this period, see Edna Heidbreder, *Seven Psychologies* (New York, 1933).

psychology's subject matter was no longer the mind. Most, however, also mistrusted uncontrolled experimentation and were convinced that introspection and the verbal report of mental events could not be the foundation of their science. As a result of this distrust and their commitment to study processes involved in the lives of organisms, many psychologists in America during the 1920s and 1930s studied learning in animals in carefully controlled settings, typified by simple models of rats learning how to run mazes. This approach, while more meaningful and complex than is often pictured, did slight the mind and its processes, concentrating on behavior that could be manipulated and objectively observed. Although many psychologists explicitly avoided any dogmatic position and Watson had few true disciples, the "normal science" of American psychology during this period was clearly behaviorist.⁵

Gestalt psychology, on the other hand, concentrated on mind and thought. It began at Frankfurt in 1910, when Max Wertheimer met with two young colleagues, Wolfgang Köhler and Kurt Koffka, and explained elegantly the well-known phi phenomenon, the phenomenon of apparent movement. The details of this work are not important here, except to note the basic conclusion: what is seen is different from what other contemporaneous perceptual theories described. Thus, the Gestaltists believed their psychology could better explain how human beings perceive things as they do. And they soon moved beyond perception to examine other mental processes.

In studying perception, the Gestaltists focused on what they called a perceptual field and dismissed as misleading the Americans' preoccupation with atomistic sensory elements. The human mind, they argued, does not see the world in bits and pieces but rather perceives wholes, or configurations, or Gestalten. In this way, the blinking bulbs of a traffic sign are seen as an arrow and are not seen individually. And this stress on relationships rather than elements led them to explain how the mind, which perceives configurations, can perceive one song when played in two different keys—that is, with *no* notes (or sensory elements) in common—as the same song.

Even when they worked in areas that the behaviorists investigated, the Gestaltists' concern with the mind and its perceptions of wholes led them to approaches and interpretations uniquely their own, quite different from the ones current in America. Köhler, for example, went to Tenerife in the Canary Islands early in the summer of 1914, planning to spend a few months studying the way in which chimpanzees learn. At the start of World War I, the Spanish government interned him, and he was forced to spend several years on the islands. During this time, he refined his experiments and developed a view that apes, and humans, learn by insight. Köhler argued that both species learn by recognizing the interrelationships among objects in the same perceptual field,⁶ not by the trial and error processes described by American psychologists. Thus, a chimpanzee who realizes that he can reach a banana outside of his cage by putting two sticks together can be said to have

⁵ *Ibid.*, 234–86. Also see Robert S. Woodworth, "Dynamic Psychology," in Carl Murchison, ed., *Psychologies of 1930* (Worcester, Mass., 1930), 327–36, and *Contemporary Schools of Psychology* (New York, 1931).

⁶ Köhler, "Zur Psychologie des Schimpansen," *Psychologische Forschung*, 1 (1921): 2–46.

achieved insight into the structure of the field in which he lives. And Gestalt psychologists argued that the same could be said for the high school student who learns how to prove theorems in geometry or the scholar who finally grasps the meaning of a text. In this way, though later, Wertheimer set out to explain “productive thinking” and strove to understand, for example, the insights that led Einstein to his theories.⁷ Here then was a powerful psychology that took the mind seriously, went beyond anything the behaviorists had to offer, and soon proved attractive to many Americans.

But Gestalt psychology was much more than a “research program.” It quickly developed beyond a simple theoretical commitment or “even an entire philosophy.” Wertheimer’s son and biographer, who calls his father a “prophet,” wrote of the Gestalt “vision [as] a *Weltanschauung*, indeed an all encompassing religion.” Koffka sometimes wrote with the fervor of a preacher, “overwhelmed with the intrinsic beauty and fruitfulness of Wertheimer’s new approach and . . . want[ing] to share the gift I received with all other psychologists.” Köhler, in his obituary of Wertheimer, described his older colleague as “a profoundly happy man, . . . filled . . . with a bliss which those who have found religion may best understand.” Late in life, he admitted the missionary aspects of the school, quoting the remark of a distinguished American neuropsychologist, “Excellent work—but don’t you have religion up your sleeve?” In all, even in its first years, the work of the Gestaltists was seen not solely as science.⁸ Instead, the attitudes of Wertheimer, Koffka, and Köhler led other psychologists to speak of a Gestalt movement.⁹

By 1920, Gestalt psychology was fairly well established in Germany. Although only an *Ausserordentlicher Professor* at Giessen, a small provincial university, Koffka had headed its psychological laboratory since 1918 and directed a thriving research program. In 1920, Köhler was appointed acting director at the Berlin Psychological Institute. He had begun to move away from experimental work to explore the theoretical implications of Gestalt psychology for physical and biological systems. In a resultant book, which he described as a “natural philosophical” study, Köhler discussed what he called physical Gestalten and theorized about the relations between the perceptual field and his hypotheses of actions of the brain. It was received well in Germany, and in 1922 he was appointed professor at the University of Berlin and director of the Psychological Institute.¹⁰ A person of independent

⁷ Max Wertheimer, *Productive Thinking* (rev. edn., New York, 1959), 213–33; Michael Wertheimer, “Relativity and Gestalt: A Note on Albert Einstein and Max Wertheimer,” *Journal of the History of the Behavioral Sciences*, 1 (1965): 86–87; and Arthur I. Miller, “Albert Einstein and Max Wertheimer: A Gestalt Psychologist’s View of the Genesis of Special Relativity Theory,” *History of Science*, 13 (1975): 75–103.

⁸ Michael Wertheimer, “Max Wertheimer: Gestalt Prophet,” *Gestalt Theory*, 2 (1980): 3–17; Koffka to Edwin G. Boring, April 22, 1930, Harvard University Archives, Cambridge, Mass., Edwin G. Boring Papers [hereafter, Boring Papers]; Köhler, “Max Wertheimer, 1880–1943,” *Psychological Review*, 51 (1944): 143–46, and “Gestalt Psychology,” *Psychologische Forschung*, 31 (1976): xviii–xxx. Also see Köhler, “A Perspective on American Psychology,” *Psychological Review*, 50 (1943): 77–79; and Molly Harrower, “Koffka’s Values and Beliefs: Expressed in a Thirteen-Year Correspondence,” paper presented at the Eighty-Seventh Meeting of the American Psychological Association, held in New York City, September 1–5, 1979.

⁹ For a later review of criticisms made earlier, see Boring, “The Gestalt Psychology and the Gestalt Movement,” *American Journal of Psychology*, 42 (1930): 308–15.

¹⁰ Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand: Eine naturphilosophische Untersuchung* (Braunschweig, 1920); and Mitchell G. Ash, “Wissenschaftsentwicklung und Disziplinstruktur: Deutschspra-

means, Wertheimer was less concerned about formal position. But in 1916 he became associated with the institute at Berlin and remained there until 1929, when he accepted the professorial chair at Frankfurt. Also at Berlin were a number of younger Gestaltists and others whose theoretical perspectives made use of the Gestalt approach. The most important of these was Kurt Lewin, a young psychologist who studied the mental development of children as the gradual differentiation and integration of perceived interrelationships among stimuli within the overall social field in which they live.¹¹ By the mid 1920s, Berlin was clearly the center of the movement. Gestaltists made an interesting and important contribution to the cultural life of Wilhelmine and Weimar Germany,¹² which, however, cannot be related here, for our subject is the Gestaltists' experience in America.

THESE SHORT SKETCHES of behavioral and Gestalt psychologies show how different they were. And yet these differences did not lead Americans to reject Gestalt ideas out of hand. On the contrary, it is a notable fact that the Gestalt psychologists were listened to, and their notions were studied and to some degree accepted. The dissatisfaction of many Americans with the extremes of behaviorism led more than a few to an interest in Gestalt ideas. But, equally important, Americans who had studied in Europe during the early years of this century already knew the Gestalt psychologists personally and considered them as friends. Herbert S. Langfeld of Princeton studied in Berlin from 1904 to 1909, where he met and befriended Koffka. In 1912, Langfeld (then at Harvard) sent one of his students, Edward C. Tolman, to Giessen, where Koffka taught as a *Privatdozent*, and the American served as a subject in some early Gestalt experiments. Eleven years later, when the opportunity next presented itself, Tolman returned to Giessen and Koffka for a few months. By 1919, Tolman had "conceived that a rat in running a maze must be learning a lay-out pattern"—a standard behaviorist experiment whose results were interpreted in Gestalt terms. He kept in continual contact with Koffka for the next two decades and his "purposive behaviorism," which stressed "cognitive maps" quite like the Gestalt perceptual fields, was one of the leading approaches to psychology in the 1930s.¹³

chige Psychologen in der Immigration" ("On the Involuntary Transfer of Knowledge: German-Speaking Psychologists in the United States, 1920–1945"), in *Berichte zur Wissenschaftsgeschichte* (forthcoming).

¹¹ Jean Matter Mandler and George Mandler, "The Diaspora of Experimental Psychology: The Gestaltists and Others," in Fleming and Bailyn, *Intellectual Migration*, 371–419. For the material collected by the Mandlers in the preparation of their paper, which provided some of the information and much of the background for this essay, see Archives of the History of American Psychology, Akron, Ohio, Jean Matter Mandler and George Mandler Papers [hereafter, Mandler Papers]. Also see Alfred J. Marrow, *The Practical Theorist: The Life and Work of Kurt Lewin* (New York, 1969).

¹² For a brilliant and detailed analysis of the Gestaltists in Weimar Germany, see Mitchell G. Ash, "The Emergence of Gestalt Theory: Experimental Psychology in Germany, 1890–1920" (Ph.D. dissertation, Harvard University, 1982).

¹³ Tolman, "Autobiography," in Edwin G. Boring *et al.*, eds., *A History of Psychology in Autobiography*, 4 (Worcester, Mass., 1952), 327–29, *Purposive Behavior in Animals and Men* (New York, 1932), and "Cognitive Maps in Rats and Men," *Psychological Review*, 55 (1948): 189–208; Koffka, "Beiträge zur Psychologie der Gestalt und Bewegungserlebnisse," *Zeitschrift für Psychologie*, 67 (1913): 353–58; and Ash, "On the Involuntary Transfer of Knowledge."

Robert M. Ogden of Cornell studied with Oswald Külpe at Würzburg from 1901 to 1903 and during the summer of 1909. He met and grew friendly with Koffka during the second of these visits and later included Gestalt ideas in some of his own work. In 1922, Ogden delivered a paper on Gestalt theory at a meeting of the American Psychological Association and solicited Koffka's first American paper, "Perception: An Introduction to the *Gestalt-Theorie*," for the *Psychological Bulletin*.¹⁴ In the early 1920s, Gordon Allport spent a year in Germany on a Sheldon Traveling Fellowship from Harvard and took advantage of this opportunity to acquaint himself with the various psychological movements that sprang up after the war. Although especially impressed with William Stern's personalistic psychology, then being developed at Hamburg, he was also taken by "the brilliance of the Lewinian approach" and thought much of "the high quality of experimental studies by the Gestalt school" in general. In 1923 he reported on the latest currents in German psychological thought in the *American Journal of Psychology* and in the following year published "The Standpoint of Gestalt Psychology" in a leading English journal on the invitation of its editor.¹⁵ None of these four Americans (Langfeld, Tolman, Ogden, and Allport), with the possible exception of Ogden, ever specifically identified himself as a Gestalt psychologist, but they all played a major role in its transmission to the United States.

By 1924, other American psychologists were studying the Gestaltists and finding aspects in the work of Köhler and Koffka exciting. Even Watson in the mid-1920s "struggle[d] with Köhler's presentation of Gestalt-psychologie [but] failed to get a kick out of it."¹⁶ In that year, English translations of two Gestaltist books were published: Koffka's *Growth of Mind* (translated by Ogden) and Köhler's *Mentality of Apes*. They did much to make Gestalt ideas more readily available to Americans.¹⁷ Both were received well by American psychologists and were widely reviewed in popular journals. Some professional reviewers were a bit cautious about some of Koffka's and Köhler's claims, but the nonpsychologists who reviewed the books had few, if any, qualms. For example, in a joint review of both books in the *Saturday Review of Literature*, Ralph Barton Perry, the Harvard philosopher, wrote of their "freshness of treatment, and [their] inventiveness and fertility of method that produce an effect very much like the opening of a window." But Perry was bothered by those aspects of the books that reflected the status of Gestalt psychology as a movement. "The theory is as yet on what might be called a war-

¹⁴ Ogden, "The Phenomena of 'Meaning,'" *American Journal of Psychology*, 34 (1923): 223–30; Koffka, "Perception: An Introduction to the *Gestalt-Theorie*," *Psychological Bulletin*, 19 (1922): 531–85; and Boring to Jean and George Mandler, June 7, 1967, February 14, 1968, Mandler Papers. Also see Frank S. Freeman, "The Beginnings of Gestalt Psychology in the United States," *Journal of the History of the Behavioral Sciences*, 13 (1977): 352–53.

¹⁵ Allport, "Autobiography," in Edwin G. Boring and Gardner Lindzey, eds., *A History of Psychology in Autobiography*, 5 (New York, 1967), 7–11, "Leipzig Congress for Psychology," *American Journal of Psychology*, 34 (1923): 612–15, and "The Standpoint of Gestalt Psychology," *Psyche*, 4 (1924): 354–61.

¹⁶ Watson to Boring, September 15, 1924, Boring Papers.

¹⁷ Koffka, *Die Grundlagen der psychischen Entwicklung: Eine Einführung der Kinderpsychologie* (Osterwieck, 1921), *The Growth of the Mind: An Introduction to Child Psychology*, trans. Robert M. Ogden (New York, 1924); Köhler, *Intelligenzprüfungen an Anthropoiden* (Berlin, 1917), *Intelligenzprüfungen an Menschenaffen* (Berlin, 1921), and *The Mentality of Apes*, trans. E. Winter (New York, 1925).

footing. It enjoys the *esprit* of an armed revolution." Yet he predicted that, "when it settles down to live peaceably with its neighbors," all psychology will benefit.¹⁸

In the Harvard psychological laboratory, then part of a joint department of philosophy and psychology, interest in Gestalt ideas began slowly but soon mushroomed. In 1923 a young graduate student named Harry Helson had to argue for a long time with his professor before he was allowed to submit a survey and interpretation of Gestalt psychology as his dissertation. When published a few years later in a series of articles in the *American Journal of Psychology*, Helson's thesis did much to acquaint American psychologists with the richness and complexity of Gestalt theory.¹⁹ Meanwhile, his professor, Edwin G. Boring, the leading experimental psychologist at Harvard, began having second thoughts about his opposition to Gestalt psychology. By 1924, his earlier well-known commitment to structuralism had begun to waver, and the various attempts to apply psychology to practical problems did not impress him. In the mid-1920s, he was open to many different views of psychology, demanding only that they be scientific—preferably experimental—and rigorous.²⁰ In 1924 he wrote to Watson of Gestalt psychology's "power to stimulate the great deal of research which I call good research and which I find very interesting." In the middle of 1925, after a summer at Harvard, a student from Vassar reported to her professor, Margaret Floy Washburn, that Boring had become a "configuralist." Boring denied the charge—"so I am a contortionist, or whatever the word is." But two months later, reporting to Koffka that "rumor is beginning to drift in that I am a Gestalt psychologist," he seemed quite impressed with the German professor's ideas. "Very well, so be it. At least what I get from Köhler, added to the little I get from you, seems to be eminently good scientific sense." But strong as this statement was, it was not as strong as one he had made more than six months earlier in a letter to Köhler. "I have decided that I am not a *Gestalt* psychologist but merely a scientist. *Gestalt* psychology seems to me to be nothing more than the introduction of science into psychology."²¹

In the mid-1920s the Gestalt psychologists began to visit the United States, and Americans could hear at first hand the ideas that excited them. To be sure, Max Wertheimer, the leader of the Gestalt school, did not cross the Atlantic during this period and, because he published much less than his colleagues, especially Koffka

¹⁸ See Perry, review of Koffka's *Growth of the Mind* and Köhler's *Mentality of Apes*, in *Saturday Review*, May 23, 1925, p. 773; and Edward L. Thorndike, review of Köhler's *Mentality of Apes*, in *The Nation*, April 15, 1925, p. 430, review of Koffka's *Growth of the Mind*, *ibid.*, May 13, 1925, pp. 553–54.

¹⁹ Helson, "E.G.B.: The Early Years and Change of Course," *American Psychologist*, 24 (1970): 625–29, "The Psychology of Gestalt," *American Journal of Psychology*, 36 (1925): 342–70, 494–526, 37 (1926): 25–62, 189–223; and interviews with Edwin B. Newman, October 3, October 26, 1979.

²⁰ John O'Donnell has shown that Boring's concern for scientific rigor led him to write his *History of Experimental Psychology* (New York, 1929), which ignored most branches of applied psychology. He used his *History* in a self-conscious attempt to influence the direction in which American psychology was developing. It can also be argued that he tried to use his vast correspondence with psychologists in the same way and that he carefully preserved more than one hundred thousand letters to make sure future historians had access to his opinions—a point that must be realized in reading his correspondence about Gestalt psychology and the Gestalt psychologists. See O'Donnell, "The Crisis of Experimentalism in the 1920s: E. G. Boring and His Uses of History," *American Psychologist*, 34 (1979): 289–95; and Boring, "Psychologists' Letters and Papers," *Isis*, 58 (1967): 103–07.

²¹ Boring to Watson, September 20, 1924; Washburn to Boring, September 16, 1925; Boring to Washburn, September 19, 1925; Boring to Koffka, November 17, 1925; Boring to Köhler, April 30, 1925, Boring Papers.

and Köhler, remained in the background as far as Americans were concerned. To many in this country, he was "the mysterious figure." Despite glowing reports of America from Koffka and Köhler and Boring's several efforts to have him appointed a visiting professor at Harvard, Wertheimer stayed in Germany until 1933. He moved first to Marienbad in Czechoslovakia but soon afterward accepted Alvin Johnson's call to the New School for Social Research in New York. Johnson, it should be noted, had studied psychology at the University of Nebraska and hated it. To him, Wertheimer's approach was just what the science needed, and his appointment of the Gestaltist was made with an eye toward a reformation of American psychology as a whole.²²

Other Gestaltists came to America much earlier, and under more pleasant conditions. For example, in 1924 Ogden arranged a visiting appointment for Koffka at Cornell, which was followed by visiting professorships at Chicago and Wisconsin. In 1925, "on the invitation of the program committee," he spoke before the American Psychological Association and took part in a well-attended "Round Table Conference on the 'Gestalt-Psychologie.'" During the next three years he gave at least thirty lectures on Gestalt psychology before various groups around the country, including the Philosophical Club at Harvard, and Boring tried to arrange for Koffka to take his place in Cambridge while he was on a sabbatical leave.²³ By late 1926 it became clear that Koffka would settle in America—he was getting a divorce from his wife, who planned to return to Germany—and rumors began to circulate about the salary offers he was receiving from various universities. By February 1927, Wisconsin had offered him a professorship with a salary of \$7,500, at a time when Boring, at Harvard, was being paid \$5,500. But within three months, Koffka had accepted a professorship at Smith College, with a salary reported to be in the neighborhood of \$10,000.²⁴

Several commentators have complained that Smith College could not give Koffka the institutional base in American psychology that a man of his stature deserved. But in many ways a research professorship at Northampton was a step up from Koffka's status at Giessen. He had no teaching obligations, a commitment (soon fulfilled) for a research laboratory to be constructed to his specifications, an adequate research budget, and a chance to appoint his own two assistants. He also had easier access to New York and Boston at Smith than he would have had at Wisconsin. And if the rumors about his salary are correct, he had a financial incentive to go to Northampton.²⁵ Meanwhile, he continued to lecture extensively,

²² See Michael Wertheimer, "Max Wertheimer: Gestalt Prophet"; interview with Edwin B. Newman, October 26, 1979; and Alvin Johnson, *Pioneer's Progress: An Autobiography* (New York, 1952), 83, 95, 114, 336, 343.

²³ Boring to Jean and George Mandler, June 7, 1967, February 14, 1968, Mandler Papers; "Proceedings of the American Psychological Association, 1924," *Psychological Bulletin*, 22 (1925): 69–138; Archives of the History of American Psychology, Akron, Ohio, Kurt Koffka Papers [hereafter, Koffka Papers], register; and Boring to James H. Woods, November 12, November 24, 1924, November 8, November 11, November 16, 1925, February 7, February 28, 1927; Boring to Koffka, March 2, March 9, 1927; Koffka to Boring, November 13, 1926, Boring Papers.

²⁴ Boring to Woods, November 8, 1926, February 28, 1927; Boring to Koffka, November 22, 1926; Koffka to Boring, February 23, April 1, 1927, Boring Papers. Also see Ash, "On the Involuntary Transfer of Knowledge."

²⁵ Seth Wakeman to Koffka, October 5, October 12, November 16, 1926, January 1, January 11, February 28, March 2, June 1, 1927, Koffka Papers.

and he and his friends continued to publish. His *Principles of Gestalt Psychology*, written in English and published in the mid-1930s, was especially influential. At Smith he even developed a small graduate program and played a major role in training such distinguished psychologists as Molly Harrower and Eleanor J. Gibson. Much later, Boring noted that Koffka "had great influence."²⁶

THE NINTH INTERNATIONAL CONGRESS OF PSYCHOLOGY, held in September 1929, at Yale, marked a watershed in the migration of Gestalt psychology to the United States. Gestalt ideas formed the theoretical perspective that attracted most attention at New Haven. Even Karl Lashley, the distinguished American neuropsychologist, made extensive use of the Gestalt point of view in his paper, "Basic Neural Mechanisms in Behavior," presented as his presidential address before the American Psychological Association, which met at the congress.²⁷ He had long since grown impatient with what Frank A. Beach, his biographer, called "the narrow, self-conscious pseudo objectivism [of] . . . the behaviorists" and had spent much time during the previous five years trying to write a book on learning theory based at least in part on ideas developed by the Gestaltists. He never finished the book, but his stress on field theory showed up in several scientific papers, including his address at the congress. This talk buoyed the spirits of younger Americans interested in the Gestalt perspective, since Lashley demonstrated that "it was possible to be a scientist and hold Gestalt views because even a jellyfish seemed to have gradients that made configurations real and tangible."²⁸

Of course, most American papers at the congress reflected the "normal science" of middle-of-the-road, applied, nondogmatic, functionally oriented behaviorism. But papers that explicitly took—or challenged—a systematic point of view concerned themselves with Gestalt psychology. Many, of course, were by Europeans, and the American congress committee had tried to attract as many Germans—and Gestaltists—to the conference as possible, often by arranging summer school positions for them to defray the costs of their visit.²⁹ Furthermore, the Gestalt psychologists were given important positions on the program. For example, only two psychologists addressed the major plenary session: Ivan Pavlov and Wolfgang Köhler. Köhler spoke "Über eininge Gestaltprobleme," and accounts of the

²⁶ Boring to Jean and George Mandler, June 7, 1967, Mandler Papers; Ogden, "The Gestalt-Hypothesis," *Psychological Review*, 35 (1928): 136–41; Ogden to Koffka, June 17, 1927, January 2, March 30, April 4, October 16, November 11, November 22, 1928, Koffka Papers; and Koffka, *Principles of Gestalt Psychology* (New York, 1935).

²⁷ *Ninth International Congress of Psychology* (Princeton, N.J., 1929); Herbert S. Langfeld, "The Ninth International Congress of Psychology," *Science*, 70 (1929): 364–68; Katherine Adams Williams, "Psychology in 1929 at the International Congress," *Psychological Bulletin*, 27 (1930): 658–63; and Lashley, "Basic Neural Mechanisms," *Psychological Review*, 27 (1930): 1–24.

²⁸ Frank A. Beach, "Karl Spencer Lashley," in National Academy of Sciences, *Biographical Memoirs*, 35 (1961): 163–204; and interview with Edwin B. Newman, October 26, 1979.

²⁹ See, for example, Langfeld to Koffka, December 16, 1927, January 20, January 27, February 15, June 4, November 7, December 15, 1928, March 8, 1929, Koffka Papers; and Boring to Carl E. Seashore, June 25, August 21, 1928, Boring Papers. Also see "Subjects of Morning Lectures and Afternoon Discussions by Six Foreign Psychologists, First Summer Session, 1929, State University of Iowa," University of Iowa Archives, Iowa City, Institute of Child Welfare Papers.

congress published in various psychological journals stressed his talk and the Gestaltists' attendance. Many Americans also spoke on Gestalt psychology. Some, like Helson and Tolman, used its approach to psychological problems, and others, like Washburn, attacked it.³⁰

Kurt Lewin, however, was the most influential Gestaltist at the congress. One of his leading students stated that the international conference did more to bring Lewin's topological approach to America than Hitler ever did. At New Haven, his presentations were great successes. For example, he showed films with titles such as "The Child and Force Fields" and "Walking Upstairs for the First Time," which illustrated the use of his field theory to analyze children's behavior.³¹ Lewin's English, unlike Koffka's, was poor, but he plunged ahead, using gestures and slang expressions to communicate. His listeners, on both formal and informal occasions, always seemed to understand him. After immigrating to the United States, his addiction to American slang—evidence of his desire to assimilate, as was the way he began to pronounce his name, Loo-in instead of Le-veen—exposed him to student pranks. At a meeting of psychologists, Lewin was heard to compliment speakers for "slobbering a bib full," because he had been told the expression meant "to give an excellent talk" or "to present some excellent ideas."³² His personality, so different from Koffka's or Köhler's, was a major reason why Americans found him attractive. But they also appreciated his psychological work. Within a month or two after the international congress, various American universities wanted to offer him visiting appointments, but it was not until 1931 that a term at Stanford was arranged for the following year. Meanwhile, he was being described as "the man of the hour" and "the young dynamo" of Berlin, and during the summer of 1931, when a group of six New England psychologists met for dinner, Lewin's psychology was the principal topic of conversation.³³

Despite Lewin's scholarly reputation and the high regard Lewis Terman, the distinguished tester, had for him and his work, Terman had to make inquiries about Lewin before a visiting appointment could be made at Stanford. For example, he wrote to Boring and asked "whether Lewin is a Jew. It would not necessarily be fatal to his appointment here if he were, but it would be best for me to know the facts if I were recommending him. The few Jews we have on our Stanford faculty have no trace whatever of the objectionable traits usually attributed to Jews, and against this kind I haven't the slightest prejudice in the world."³⁴ To answer Terman's question, Boring had to ask around and finally learned from a

³⁰ *Ninth International Congress*, 270–72; Helson, "The Effects Obtained from Rotation of Irregularly Formed Regions," in *ibid.*, 219–20; Tolman, "Maze-Function of Motivation and of Reward as well as of a Knowledge of the Maze-Paths," in *ibid.*, 439–40; Washburn, "Köhler's Discussion of Association," in *ibid.*, 470; and Langfeld, "The Ninth International Congress," 364–68.

³¹ Interview with Tamara Dembo, November 14, 1973; and Boring to Terman, July 31, October 6, 1931, Stanford University Archives, Stanford, Lewis Terman Papers [hereafter, Terman Papers]. Also see Gordon W. Allport, "The Genius of Kurt Lewin," *Journal of Personality*, 16 (1947): 1–10; and Grace Heider, "News and Notes," *Newsletter of the Division of the History of Psychology*, 7 (1974): 10.

³² Miriam Lewin, "In My Father's Steps," paper presented at the Eighty-Sixth Meeting of the American Psychological Association, held in Toronto, August 28–September 1, 1978.

³³ Mandler and Mandler, "The Diaspora of Experimental Psychology," 400–05; and Boring to Terman, July 31, 1931, Boring Papers.

³⁴ Terman to Boring, August 13, 1931, Boring Papers.

former student, Carroll Pratt, who had recently studied in Berlin, that Lewin *was* a Jew. Lewin was appointed, nevertheless, and his tenure as a visiting professor at Stanford was successful from all points of view. Terman, his students, and colleagues were not only greatly impressed with Lewin's psychological ideas and teaching but also grew to like him. Of all of the Gestaltists, Lewin was the least formal—in Boring's words, "anything but self-important after the German manner." Even in Germany, where his "careful systematic" research and writing was followed "with the greatest respect by everyone" and he had "a great many students working under him," Lewin "seemed to be on the most friendly terms with everyone . . . a most agreeable person."³⁵ In 1933, when Hitler came to power, Lewin was visiting in Japan and cabled both Boring and Terman in hopes of getting a position in the United States. Both were concerned about the situation, and Terman's words about what Lewin meant to him stand in contrast to his earlier inquiries about his ethnic background. "I have also been intending to write to you particularly to tell you how highly we appreciated Lewin. . . . His work commanded the respect of our students, both graduate and undergraduate, and of our department faculty. . . . Faculty and students became so fond of him that it was hard to let him go. I have known few people who were so alive to everything about them, or so genial and friendly."³⁶

Neither Boring nor Terman was able to get Lewin a position in the United States, despite Terman's personal appeal to Alvin Johnson and some frantic if ineffective efforts on the part of Boring and Koffka. Ogden finally arranged a position for Lewin in Cornell's School of Home Economics, which allowed him to continue his studies of child development in the nursery school. From Cornell he went to the Iowa Child Welfare Research Station, where he continued his work with children, and in the early 1940s he moved to MIT, where he established a research center for group dynamics. Previous writers who have overstated the problems the Gestaltists encountered in this country have complained that Lewin never held a permanent position in a traditional academic psychology department. Lewin, however, was able to carry out his research wherever he found himself and at least as well as he had at the Berlin Psychological Institute. It is doubtful that Lewin worried about issues of status. In any event, it is hard to overstate his influence on American psychology.³⁷

TO MANY PEOPLE IN THE UNITED STATES, however, Gestalt psychology in the 1920s and early 1930s meant primarily the work of Wolfgang Köhler, and the history of his interaction with American psychologists through the mid-1930s reveals much about the way in which Gestalt ideas and the Gestalt psychologists were received.

³⁵ Boring to Terman, July 31, August 18, October 6, 1931; Terman to Boring, November 19, 1934, Terman Papers.

³⁶ Boring to Terman, April 7, 1933; Terman to Boring, April 11, 1933, Terman Papers.

³⁷ Terman to Johnson, June 12, 1933, Terman Papers; Boring to Koffka, April 5, April 7, May 2, May 15, May 18, 1933; Koffka to Boring, April 6, May 1, May 16, 1933, Boring Papers; Mandler and Mandler, "The Diaspora of Experimental Psychology"; and Dorwin Cartwright, "Contemporary Social Psychology in Historical Perspective," *Social Psychology Quarterly*, 42 (1979): 82–93.

This fact, as well as the many rumors that still circulate concerning Köhler's relations with several American psychologists, especially those at Harvard, justifies an extensive treatment of his role. It is not generally known that Köhler had contacts with American psychology as early as 1914, when he was working with chimpanzees at Tenerife. Early that year, Robert M. Yerkes, then a young assistant professor at Harvard, wrote to Köhler, expressing interest in his work, asking for further information about it, and hoping to be able to join the German professor off the coast of Africa. The outbreak of the European war and Köhler's internment on Tenerife ended Yerkes's travel plans. But the two psychologists soon began to exchange offprints, and the American even asked John B. Watson to send Köhler a set of his articles. Yerkes also had Köhler's motion picture films of his chimpanzee experiments processed in the United States when this became impossible on the islands. But this friendly and mutually profitable exchange was marred in 1916, when Yerkes published *The Mental Life of Monkeys and Apes* and did not cite Köhler's work. And with American entry into the war in 1917, the relationship came to a temporary close.³⁸

Shortly after the war, Yerkes used his temporary position with the National Research Council to try to re-establish contact with Köhler. He accomplished this by 1921, and soon the two psychologists exchanged books, offprints, and congratulations on each other's appointments: Köhler's at Berlin and Yerkes's at Yale. Yerkes even offered to send money to Köhler to cover the cost of the books and offprints, in view of the deterioration of the economic situation in Germany and "the unfairness of the exchange situation," but Köhler would not accept. By 1923, the two men were again learning much from a correspondence they both apparently enjoyed. And in 1924, Yerkes reviewed Köhler's *Mentality of Apes* most positively, writing that both its "observations and conclusions . . . are important" and hoping "that it may also achieve wide influence."³⁹

Later in 1924 Carl Murchison at Clark University arranged for Köhler to serve as visiting professor at the Worcester institution during 1925, and the reaction of many American psychologists was enthusiastic. Terman, a Clark alumnus who had been bemoaning the condition of psychology at his alma mater, was especially pleased. As he wrote to a Clark official, "It was a splendid stroke to get Dr. Köhler to come over." Boring and Yerkes congratulated Köhler on his appointment and were among the first to welcome him to America. Boring, of course, had been studying Gestalt psychology at the time, and his welcome was particularly enthusiastic. "The psychological stock of America took a jump upward as soon as I heard you were safely on shore."⁴⁰ Once Köhler was at Worcester, Boring and Yerkes saw him regularly, and the visitor spoke at least once at both Harvard and Yale. Boring even

³⁸ Yerkes to Köhler, March 27, May 20, 1914, July 17, October 10, December 21, 1916, January 10, 1917; Köhler to Yerkes, April 17, 1914, February 15, May 19, 1916, September 10, 1917, December 13, 1916, March 2, 1917, Yale University Archives. New Haven, Robert M. Yerkes Papers [hereafter, Yerkes Papers].

³⁹ Yerkes to U.S. Department of State, February 14, 1921; Yerkes to Köhler, April 13, October 8, 1921, March 6, 1922, November 6, 1923; Köhler to Yerkes, May 19, October 23, 1921, January 29, 1922, Yerkes Papers; and Yerkes, review of Köhler's *Mentality of Apes*, in *International Book Review*, June 1925, p. 461.

⁴⁰ Terman to C. H. Thurber, May 26, 1924, Terman Papers; Boring to Köhler, June 9, 1924, February 6, 1925, Boring Papers; and Yerkes to Köhler, October 22, 1924, January 26, 1925, May 7, 1924; Köhler to Yerkes, February 8, 1924, February 15, 1925, Yerkes Papers.

attended weekly seminars led by Köhler at Clark, which he described as “great fun,” at least in part because the meetings included extensive discussions of Köhler’s experimental work. These seminars in turn led to Köhler’s influential chapters in *Psychologies of 1925*, published by Clark University Press.⁴¹ Meanwhile, Köhler met with other members of the American psychological community and impressed most of them. He and Koffka attended a meeting of the Society of Experimental Psychologists as guests, two of the few ever so honored (see Figure 1). During a later visit to Stanford, Köhler impressed Terman as “an intellectually active man” with “youth and vigor.” Yerkes also recommended Köhler and Koffka to the home secretary of the National Academy of Sciences as “two of the foremost German psychologists” in an effort to get for them a place on the academy’s programs. In 1926 Ogden sought the translation and publication in English of other books by Köhler. Although unsuccessful, the attempt shows how important to American psychology Köhler’s ideas had become.⁴²

Soon after Köhler started lecturing at Clark in January 1925, Boring and James H. Woods, chairman of the joint Department of Philosophy and Psychology at Harvard, proposed to invite Köhler to Cambridge the following fall as visiting professor. The idea had the support of Harvard philosophers—Ralph Barton Perry was then reviewing favorably *Mentality of Apes*, and Woods even told the Harvard administration that he himself would raise the funds to pay for Köhler’s salary. As director of the Harvard Psychological Laboratory, Boring exchanged letters with Köhler about the position. Köhler was interested, for he wanted to work with Harvard graduate students.⁴³

But in late April 1925 Köhler spoke at Cambridge under the auspices of the Harvard Philosophical Club, and his talk disappointed Boring greatly. He later described it as being full of “general theoretical analogies [and] unformulated psychological events [with] not [one] bit of experimentation.” In another letter, he expressed dissatisfaction with the “vagueness of [Köhler’s] speculation.” He had hoped to find in Köhler the experimentalist that he strongly believed the Harvard department needed, only to discover that the Gestaltist was moving away from the laboratory.⁴⁴

⁴¹ Boring to Yerkes, October 28, 1925; Yerkes to Köhler, May 7, 1925, Yerkes Papers; Boring to Köhler, March 31, 1925; Köhler to Boring, April 2, April 20, 1925, Boring Papers; and Köhler, “An Aspect of Gestalt Psychology,” and “Intelligence in Apes,” in Carl Murchison, ed., *Psychologies of 1925* (Worcester, Mass., 1926), 129–43, 145–61.

⁴² Terman to Boring, January 26, 1927, Terman Papers; Yerkes to David White, March 5, 1925, Yerkes Papers; Kegan Paul Trench Trubner and Company to Köhler, November 16, 1926, January 19, February 15, June 14, July 22, October 11, 1927, American Philosophical Society Library, Philadelphia, Wolfgang Köhler Papers [hereafter, Köhler Papers]; and Boring, “The Society of Experimental Psychologists, 1904–1938,” *American Journal of Psychology*, 51 (1938): 410–21. The photograph on the cover of this issue and the photograph included in my essay were taken at the 1925 meeting of the Society of Experimental Psychologists. This exclusive group, first brought together in 1904 by Edward B. Titchener of Cornell and not formally organized until after his death in 1927, limited attendance at its meetings to the directors of American university psychological laboratories and their entourages of junior colleagues and graduate students. Attendance in 1925 was higher than at any previous meeting; American psychologists came both to celebrate the dedication of Eno Hall, the new psychological laboratory at Princeton, and to meet with and hear Köhler and Koffka, the honored guests.

⁴³ Woods to Abbot Lawrence Lowell, March 17, 1925, Harvard University Archives, Cambridge, Mass., Abbot Lawrence Lowell Papers [hereafter, Lowell Papers]; and Boring to Woods, April 15, April 16, 1925; Boring to Köhler, March 25, March 31, 1925; Köhler to Boring, April 2, 1925, Boring Papers.

⁴⁴ Boring to Koffka, April 23, 1930; Boring to Raymond H. Wheeler, March 3, 1927, Boring Papers.

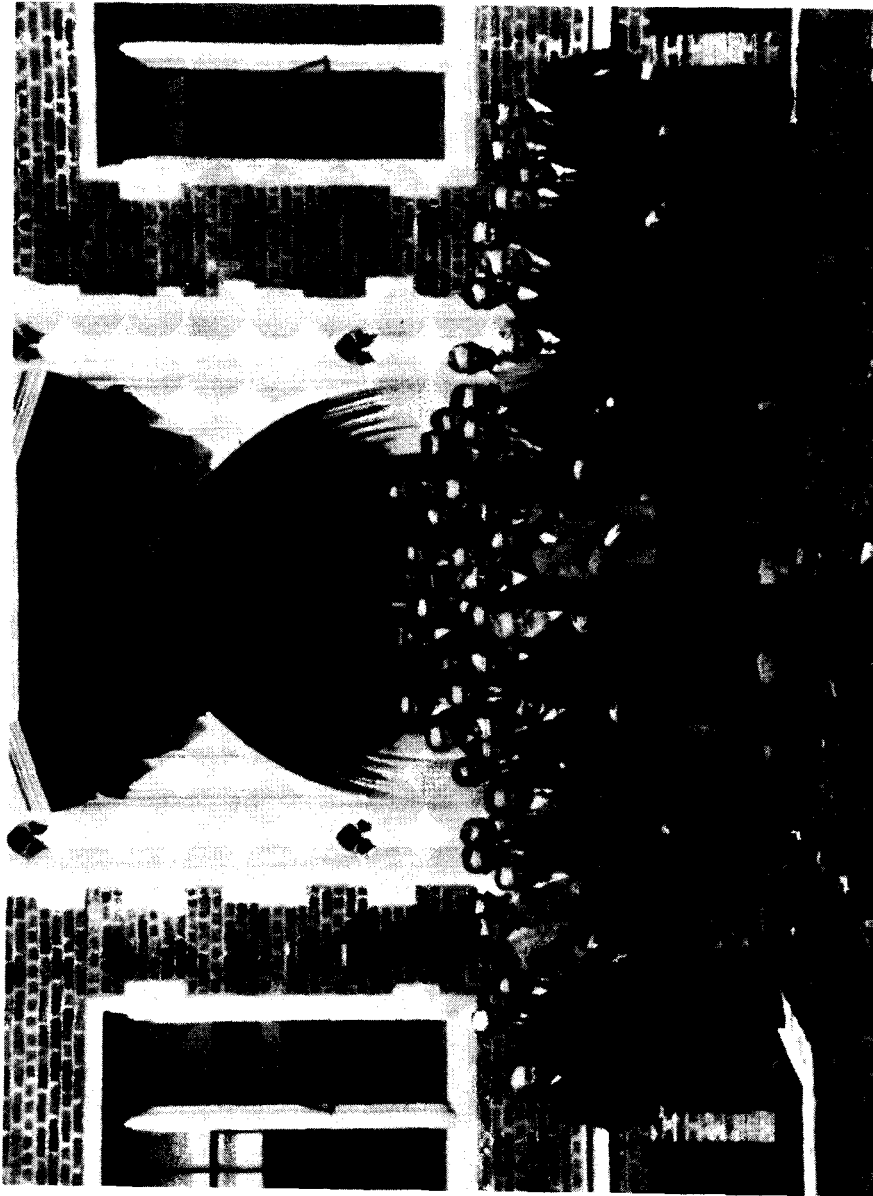


Figure 1: The 1925 meeting of the Society of Experimental Psychologists. Front row, left to right: Edwin G. Boring, Harvard University; Herbert S. Langfeld, Princeton University; Howard C. Warren, Princeton University; Edward B. Titchener, Cornell University; Kurt Koffka, visiting professor, Cornell University; Wolfgang Köhler, visiting professor, Clark University; Walter B. Pillsbury, University of Michigan; John E. Anderson, University of Minnesota; unidentified. Photograph reproduced from the collections of the Archives of the History of American Psychology, University of Akron, Akron, Ohio.

Personal concerns may also have figured in Boring's reversal. He was a moody and oversensitive man, highly conscious of his status as Harvard's leading psychologist, which would have been threatened by Köhler's appointment. But at the same time he was jealous of Harvard's position in the American psychological community and knew that it would be generally enhanced if Köhler lectured at the university.⁴⁵ His continuing correspondence with Köhler mirrors Boring's ambivalence. In June he listed in detail the facilities available at Harvard, and added, "I am very anxious for you to come." And yet he stressed the space and financial limitations at Harvard and feared Köhler might "be disappointed" at not having "what you could have had at Berlin." In response Köhler began to express doubts about the visiting appointment and finally begged off in the summer of 1925, citing his responsibilities to his colleagues in Berlin that prevented an extension of his leave. "Just about the time that Köhler got an inkling that Harvard might do something to get him here," Boring wrote several years later, "he stiffened up towards me."⁴⁶

Köhler's relations with Boring were undoubtedly "stiff." Boring's ambivalence probably set him on edge, but more than Boring's attitude was involved. Köhler was a difficult man to get to know, and he, too, enjoyed status as a professor at Germany's leading university. He believed his psychology vastly superior to any yet developed and urged its merits on Americans with a confidence that some saw as arrogance. He had high standards of propriety and was easily put off by anything he considered improper. According to an American student in Berlin, even his German friends thought him "a little too temperamental" and inclined to be rigidly formal on even informal occasions. Before transcending the formal "Sie" to the intimate "Du," Köhler had to know somebody socially for ten years or more. American friends attributed Köhler's constraint to discomfort in speaking English, his unbending need to do the proper thing socially, and his basic shyness. Whatever its cause, it did "put off" many Americans and complicated his relations with many people in this country, especially Boring, whose own personality did not ease the situation. The two men apparently never understood one another and found the negotiations difficult. Both tried to be cordial and conciliatory throughout their correspondence, and their letters to each other were always polite. But Boring, at least, disliked Köhler and felt disliked in turn. The depth of his feelings bothered him, however, and Boring once even wrote of them to his colleague, Perry, describing his letter as "the confession of a divided neurotic soul."⁴⁷ But neurotic or not, the relationship between the two men affected the history of American psychology.

Despite the personal feelings involved, Köhler did not close the door in the summer of 1925 to a visiting position at Harvard and hinted that he would welcome

⁴⁵ Boring, *Psychologist at Large: An Autobiography and Selected Essays* (New York, 1961); and Julian Jaynes, "Edwin Garrigues Boring, 1886–1968," *Journal of the History of the Behavioral Sciences*, 5 (1969): 99–112.

⁴⁶ Boring to Köhler, June 4, 1925; Köhler to Boring, July 12, 1925, Boring Papers; Boring to Yerkes, October 28, 1925, Yerkes Papers; Boring to Wheeler, March 3, 1927, Boring Papers; and Köhler to Woods, [July 10, 1925], Köhler Papers.

⁴⁷ Mandler and Mandler, "The Diaspora of Experimental Psychology"; interview with Edwin B. Newman, October 3, 1979; and Carroll C. Pratt to Boring, April 19, 1931, as quoted in Boring to Terman, July 31, 1931; Boring to Perry, [1928], Boring Papers.

an invitation to Cambridge in the future. Boring and Woods apparently discussed this possibility with Abbot Lawrence Lowell, Harvard's president. Köhler's fame in America in the mid-1920s was such that he had already been recommended strongly to Lowell by a member of the university's Board of Overseers. Henry Osborn Taylor, the New York philosopher and historian, had described Köhler in a letter to Lowell as "a light in a dark night." Lowell, intrigued by these recommendations, made inquiries of his own, writing to Edward Bradford Titchener, the leading structuralist. Titchener responded that he had been impressed by Köhler's experimental work in Germany before World War I but found his later theoretical work so disappointing that he feared "Köhler's health may have been seriously impaired by his four years' residence on Tenerife." Furthermore, Köhler in person impressed Titchener even less favorably, appearing "apathetic and uninspiring," a reaction shared, he said, by other Americans. The Cornell psychologist warned Lowell against offering Köhler a permanent chair but thought that a one-year visiting professorship would be a good test of the Gestaltist's health and ability.⁴⁸ Boring was not the only psychologist of the mid-1920s who believed that Köhler's major scientific achievements lay behind him.

Lowell apparently did not tell Boring and Woods of Titchener's opinion, and in January 1926 the two men again made plans to approach Köhler about a visiting professorship. Although these plans fell through,⁴⁹ a possibility appeared in December for a permanent appointment. In that month William McDougall, the English psychologist who had been teaching at Harvard since 1920, resigned his position and left Cambridge for Duke University. Woods immediately thought of Köhler as McDougall's successor and was supported by Perry and other Harvard philosophers. Boring, however, told his colleagues that what Harvard psychology required was a leading experimentalist and that Köhler, having left the laboratory for theoretical exploration, did not fill this need. He strongly favored the appointment of an American, such as Lashley or Tolman. He also told Perry, and perhaps others, of his personal feelings about Köhler. But he still believed that the Gestaltist's appointment at Harvard would do much for the university's standing in psychology, and, as he phrased it, he did not "think that [Köhler's] dislike of me . . . [was] a reason for not calling him, but [was] rather sorry to have this personal reaction to render me unsure of my other professional judgment." He therefore went along with Woods and Perry and agreed to ask Köhler to join the Harvard faculty, at least on a temporary basis.⁵⁰

Köhler, however, did not respond to two of Boring's letters on the subject. A third letter from Boring, and one from Woods, did bring a reply, but it did not clarify the situation. To be sure, some of the correspondence between these three

⁴⁸ Taylor to Lowell, February 28, 1926; Lowell to Taylor, March 2, 1926; Lowell to Titchener, April 17, 1926, Lowell Papers; and Titchener to Lowell, April 19, 1926, Cornell University Archives, Ithaca, N.Y., Edward Bradford Titchener Papers [hereafter, Titchener Papers].

⁴⁹ Boring to Woods, January 6, January 21, March 18, 1926, Boring Papers; and Köhler to Woods, March 15, 1926, [April 1926], Köhler Papers.

⁵⁰ Boring to Perry, [1928], Boring Papers; Woods to Boring, December 17, 1926; Boring to Woods, December 27, 1926, Boring Papers; Boring to Terman, January 17, 1927, Terman Papers; and Boring to Clarence I. Lewis, August 14, 1927, December 4, April 4, 1928, Harvard University Archives, Cambridge, Mass., Department of Philosophy Papers [hereafter, Department of Philosophy Papers].

men has apparently been lost or destroyed, so the details of the episode are unclear. Those letters that survive are polite and even friendly, and yet those who saw all the correspondence were not sure of what Köhler, who hated financial negotiations connected with jobs, intended. Boring wrote to Terman, "My own interpretation of the correspondence is that he will not accept . . . [but] some at Harvard interpret the same letters as meaning that he is coming." The one letter from Köhler to Boring that survives illustrates the problem of determining the former's intentions. "When I think of you and my other friends at Harvard the choice looks simple. But when I look at the economic situation and the number of lectures to which an American Professor tends to be obligated, then my face drops."⁵¹ Not until the fall of 1927 did Köhler definitely notify Harvard that he would stay in Berlin. Apparently he apologized to Boring for the long suspense. In reply Boring tried to be conciliatory, but his annoyance is evident. "After all what is Harvard against Berlin? . . . That you have left us in the lurch and we are still limping along is certainly not your fault, but is entirely our responsibility." It is no wonder then that he later described this period as "the summer that Köhler blew up on us."⁵²

Harvard still had to appoint a successor to McDougall, and the philosophers continued to urge that a European be chosen for the position. They kept hoping that Köhler might be persuaded to come to Cambridge. But Boring still argued for an American and an experimentalist, and he found that other American psychologists shared his, and Titchener's, low opinion of Köhler's post-1920 work, which was too grandly speculative for their tastes. Terman noted, "I doubt . . . he will ever do much more experimenting." Boring complained to his philosophical colleagues about their preference for European psychologists, who performed "the scissors and cardboard kind of experiments that do not reflect favorably upon Harvard's psychology in America." His concern for Harvard's reputation meshed with his antipathy toward Köhler's work. He cited the "criticism from the men whose opinion I respect and in whose judgment I concur." In the spring of 1928 Köhler's name was again mentioned for the position at Harvard, and Boring blew up. "American psychologists who felt that Harvard had made a mistake with both [Hugo] Münsterberg and McDougall would feel that it again erred."⁵³ But Boring's outburst got him nowhere, since many Harvard philosophers wanted Köhler. In the fall of 1928 he analyzed the situation:

The issue is out in the open. It is between A and B.
 A. Breadth of interest, vision and imagination.
 B. Technical skill and knowledge within a given field.
 We want both. We can not have them. Actually they are negatively correlated. . . .
 Hocking, Perry and Woods are for A and thus for a renewal of the offer to Köhler.
 I am for B and thus can not conscientiously agree to Köhler.⁵⁴

⁵¹ Boring to Terman, July 11, 1927, Terman Papers; and Boring to Woods, January 13, 1927; Köhler to Boring, May 13, 1927 (my translation from the German), Boring Papers.

⁵² Boring to Köhler, January 6, 1928, Boring Papers; and Boring to Terman, August 9, December 14, 1927, Terman Papers. Also see Boring to Lewis, August 2, August 14, 1927, Department of Philosophy Papers.

⁵³ Terman to Boring, January 26, 1927, Terman Papers; and Boring to Lewis, December 4, December 7, 1927, April 4, October 30, 1928, Department of Philosophy Papers.

⁵⁴ Boring to Lewis, October 30, 1928, Department of Philosophy Papers.

Boring was also annoyed that philosophers were trying to tell psychologists how to manage their own affairs. His protests over the way in which the entire situation had been handled were later a major reason why philosophy and psychology at Harvard were reorganized in the mid-1930s as separate departments within a joint division.⁵⁵

MEANWHILE, BORING HAD BEEN DEVOTING all of the time that he could spare from Harvard matters to the study of the history of psychology, in preparation for the writing of his well-known book, *History of Experimental Psychology*, published in 1929. This research, his quarrels with his Harvard colleagues, and his feelings about Köhler's experimental work, all led him to reconsider his early enthusiasm for Gestalt psychology, which he had never expressed in print. Several of his friends—for example, Margaret Floy Washburn—had always been critical of the school, and Boring had always been sensitive to the opinions of other psychologists. Some thought highly of the work of individual Gestaltists but criticized that of others. Terman, for example, found Lewin's work exciting but had qualms about Koffka's and Köhler's. He described a talk by Koffka at Stanford as "piffle" and, though somewhat impressed by Köhler's early writings, thought little of Köhler's future as an experimental psychologist. Terman felt that propagandizing by Gestalt psychologists detracted from the merit of their ideas, and Boring began to be concerned about this aspect of his relationships with Koffka and Köhler.⁵⁶

Gestaltists were not the only psychologists of the 1920s to argue that the school to which they belonged possessed the only valid approach to psychology. And yet American psychologists were especially bothered by the attitude of the Gestaltists. As shown earlier, some felt that the Gestaltists had come to the United States almost as intellectual missionaries, spreading a new gospel. And Köhler's stiffness did nothing to ease the situation. Recently the term "Mandarin" has been used to characterize the attitudes and behavior of many of the German university professors of the period. In some ways the entire Gestalt movement represented a revolt against traditional German university culture, but in other, deeper ways the Gestaltists shared many traits typical of the faculties of German universities.⁵⁷ Although they never heard the term "Mandarin" applied to the Gestaltists, many American psychologists would have easily recognized the characterization.

When Boring's *History of Experimental Psychology* appeared, his ten-page discussion of Gestalt psychology was negative in tone. Apparently he felt that he had tried to be fair. "It is a question as to whether I have been favorable or unfavorable." But friends of Gestalt psychology like Ogden had little doubt about Boring's actual opinion. His analysis of the school continually stressed its origins as a "psychology of protest" against the older, atomistic theories of psychology. Although he admitted

⁵⁵ Boring to Perry, November 15, 1928, Department of Philosophy Papers.

⁵⁶ Terman to Boring, August 3, January 26, 1927, Terman Papers; Washburn, "Gestalt Psychology and Motor Psychology," *American Journal of Psychology*, 37 (1926): 516–20; and Washburn to Boring, September 16, 1925; Boring to Carl E. Seashore, June 25, August 21, 1928, Boring Papers.

⁵⁷ Fritz K. Ringer, *The Decline of the German Mandarins: The German Academic Community, 1890–1933* (Cambridge, Mass., 1969); and Ash, "The Emergence of Gestalt Theory."

that, “if this negative element were all that there is to *Gestalt* psychology, it would never have become an important movement,” most of his discussion revolved around its criticisms of older ideas. Furthermore, he emphasized the continuity of Gestalt ideas with older theories and criticized the Gestaltists because they “made little effort to show the antiquity of the[ir] objection.”⁵⁸ One can readily see why Ogden felt attacked.

The year 1929 also saw the publication of Köhler’s *Gestalt Psychology*, which he wrote in English and intended as a general introduction to his science for American psychologists. To show his readers what Gestalt psychology was not, he opened his presentation with a long critique of behaviorism. But the psychology that these chapters attacked was almost a caricature of the American science, since they neither mentioned (with one exception in a single sentence) any behaviorist nor included a detailed discussion of any behaviorist study, even by way of example. (A bibliography did list three works by behaviorists, but only one by Watson, and that a short article.) The book also included chapters on sensory organization, association, recall, and insight, which explained quite clearly the Gestalt perspective on these topics.⁵⁹ The volume was at least partially successful, but it did not win many friends for the Gestalt school.

Although not a behaviorist, Boring believed that he had to respond to the book, and he criticized Köhler’s vague and slanted account of the American school. But he made sure to comment favorably on the most substantive chapters. Boring’s review was entitled “The *Gestalt* Psychology and the *Gestalt* Movement,” and it praised the former, while criticizing the latter. But despite his belief that Köhler’s book required his response, he felt uneasy about it and attempted, before it was published, to soften its impact. He asked a friend to read a draft to make sure it was not too “violent.” He sent galley proofs of the note to Köhler in Berlin and tried in a cover letter to explain his ambivalence. “Sometimes I seem to be so enthusiastic about it and sometimes so negative.” He wrote of his “enthusiasm for the research that has come out under this label” but admitted, “I get very angry about the label of *Gestalt* psychology and its solidarity as a new movement.”⁶⁰

If Köhler responded to Boring’s letter or to the review, his answer has been lost, but Koffka’s reaction was calm, reasoned, and cordial. He argued “that the *Gestalt movement* has been created not by the Gestalt psychologists but by their opponents” and claimed that “many misunderstandings may have been caused by the fact that Köhler and I were invited to give so many public lectures,” from which “concrete details” had to be omitted. There was some validity to Koffka’s points, especially in view of the long list of papers criticizing Gestalt psychology that Boring presented in his history, and Koffka’s attitude toward his public lectures might certainly explain why Terman thought the one he heard was “piffle.” Boring found at least some truth in Koffka’s rebuttal. But at the same time he harked back to the talk that

⁵⁸ Boring to Köhler, February 27, 1930, Boring Papers, and *A History of Experimental Psychology* (New York, 1929), 570–80, 591–93.

⁵⁹ Köhler, *Gestalt Psychology* (New York, 1929).

⁶⁰ Boring to Joseph Peterson, January 13, 1930; Boring to Köhler, February 27, 1930, Boring Papers, and “The *Gestalt* Psychology and the *Gestalt* Movement.”

Köhler had given in 1925 at the Harvard Philosophical Club, which had impressed him so unfavorably. Boring made sure to stress that these specific criticisms did not apply to Koffka, but, though cordial, Boring clearly was unhappy with Gestalt psychology, or at least with the Gestalt school.⁶¹

Boring's note was not the only response to Köhler's book critical of the Gestalt movement. The *New Republic*, for example, published an article by Edward S. Robinson, a Yale psychologist, on "A Little German Band," which was subtitled "The Solemnities of Gestalt Psychology." The essay was important in that it apparently marked the first time that Köhler and Koffka were referred to in print as Gestaltists. Previously Americans called them Gestalt psychologists or Gestalt theorists, but after its appearance in this article "Gestaltist" passed into general usage, analogous to behaviorist or structuralist and usually, despite Robinson's intent, without disparaging implications. But Köhler and other Gestalt psychologists always saw the term as an insult to them and their work and decried its use. Boring avoided it, at least partly in deference to their feelings. But even today, when many psychologists speak fondly of their teachers as Gestaltists, those still annoyed by the term blame its coinage on Boring.⁶²

By clarifying Gestalt psychology and its concepts for an American audience, Köhler's *Gestalt Psychology* made it easier for behaviorists and other non-Gestalt psychologists to attack his point of view. And in the following years, such attacks multiplied. For example, an article on "The Phantom of the Gestalt" concluded that "the Gestalt . . . has no assignable value in psychological description nor any real existence within the experimental sequence." Similarly, in "Materializing the Ghost of Köhler's Gestalt Psychology," an American psychologist argued that Gestalt concepts, if reinterpreted within the framework of Washburn's motor theory of consciousness, would be immediately subsumed within the mainstream of American psychology. These critiques and others like them are difficult to interpret, but all indicate a growing familiarity with, if not a deep understanding of, Gestalt ideas among American psychologists. By 1933, when it was included as one of the *Seven Psychologies* in Edna Heidebreder's classic survey of American schools of psychology, Gestalt psychology was clearly a part of the American psychological scene.⁶³

Meanwhile, Koffka remained at Smith, Boring at Harvard, and Köhler at Berlin. These three men continued to correspond regularly with each other, and all went on with their scientific work. Boring drifted toward behaviorism and published *The Physical Dimensions of Consciousness*, an attack on dualism that he later admitted was "immature" and a friendly colleague called a "silly little thing." Koffka began work

⁶¹ Koffka to Boring, April 22, 1930; Boring to Koffka, April 23, 1930, Boring Papers; and Boring, *History of Experimental Psychology*, 593.

⁶² Robinson, "A Little German Band," *New Republic*, November 27, 1929, pp. 10–14. James R. Angell, president of Yale and a psychologist, apparently criticized Robinson sharply for the tone of his review; see Robinson to Angell, December 10, 1929, Yale University Archives, New Haven, Conn., James R. Angell Presidential Papers. Also see Grace Heider to Jean and George Mandler, December 21, 1927, Mandler Papers.

⁶³ Frederick H. Lund, "The Phantom of the Gestalt," *Journal of General Psychology*, 2 (1929): 307–23; F. M. Gregg, "Materializing the Ghost of Köhler's Gestalt Psychology," *Psychological Review*, 39 (1932): 257–70; and Heidebreder, *Seven Psychologies*. Also see William McDougall, "Dynamics of Gestalt Psychology," *Character and Personality*, 4 (1930): 232–44, 319–34; and S. C. Fisher, "A Critique of Insight in Köhler's Gestalt Psychology," *American Journal of Psychology*, 43 (1931): 131–35.

on his very influential book, *Principles of Gestalt Psychology*, which went far beyond Köhler's introduction. In Germany, the rise of Hitler did not immediately affect Köhler, as he was an "Aryan," but he greatly disliked the Nazis and what they stood for and was one of the few non-Jewish scientists in Germany to oppose the regime actively.⁶⁴ In the mid-1930s developments at Harvard led to another invitation to Köhler. The Division of Philosophy and Psychology sponsored the William James lectures and decided—after hearing two distinguished American philosophers, John Dewey and Arthur O. Lovejoy—to invite a foreign, or at least foreign-born, psychologist as the next lecturer. After considering Carl Gustav Jung, the faculty soon focused on Koffka and Köhler. In comparing the two, the Harvard psychologists found that each had different strengths; they had to determine just what they wanted. As Boring summarized their deliberations, the Harvard professors decided "that the most distinguished and appropriate appointment that could be made at the present time would be Köhler and that the most useful appointment in the effect it would have upon the atmosphere of the laboratory would be Koffka. I have lent my support to the view that we must in such an appointment weight distinction very heavily, and that we ought therefore to ask Köhler."⁶⁵

In December 1933, Harvard invited Köhler to be the third William James lecturer, to deliver a course of ten or twelve public lectures, and to conduct a seminar for graduate students. Boring wrote to Köhler that *both* the philosophers and the psychologists in the division wanted the Gestaltist to accept the appointment and that he was especially hopeful of hearing Köhler again. Köhler responded as he had to so many previous invitations from Harvard, writing that he would love to accept "this invitation, which I regard as an unusual honour," and stressing that "it should have been impossible for me to accept without knowing about your point of view." He did not agree to the proposal immediately, citing the difficult problems he faced at the University of Berlin with regard to the Nazi-controlled administration.⁶⁶

But Köhler soon afterwards accepted and arrived at Cambridge in September 1934. Boring attended the lecture series and, burying his misgivings, apparently hoped to see a good deal of Köhler, arranging at least one social event for the visitor and his wife. But on the whole he was greatly disappointed; Köhler spent most of his time with Harvard's distinguished philosophers, especially Perry. In planning for Köhler's visit, Boring assumed that the Gestaltist would not need an office near the psychology laboratory, but his colleagues in philosophy convinced him otherwise. Once in Cambridge, Köhler hardly used the office, and the two psychologists rarely met. By November, the situation became so bad that Boring felt he had to write to Köhler, if he was to have an opportunity to discuss some points raised in the lectures. "Things are so disposed that we are not thrown together for conversa-

⁶⁴ Boring, *The Physical Dimensions of Consciousness* (New York, 1933); Jaynes, "Edwin Garrigues Boring"; Mary Henle, "One Man against the Nazis—Wolfgang Köhler," *American Psychologist*, 33 (1978): 939–44; and Ash, "The Struggle Against the Nazis," *ibid.*, 34 (1979): 363–64.

⁶⁵ Boring to Perry, March 22, October 14, 1933, Boring Papers.

⁶⁶ Köhler to Boring, January 22, 1934; Boring to Köhler, December 7, 1933; Boring to Koffka, December 13, 1933, Boring Papers.

tion.”⁶⁷ In all, Boring seems to have come across as a pushy American, and Köhler seems to have retreated into his personal shell. Both psychologists acted with the best intentions, but neither knew how to improve the relationship.

Boring had other reasons to be annoyed. The lectures that Köhler gave—published four years later as *The Place of Value in a World of Fact*—extended various philosophical implications of Gestalt theory into the realm of ethics and aesthetics and focused much attention on epistemological and metaphysical problems. Köhler’s English was good, but the lectures were not as easily followed as were the substantive chapters of *Gestalt Psychology*, which perhaps merely reflected the complexity of the topics on which he spoke. Later reviews in philosophy journals and the popular press praised the book as, for example, “keen, wide-ranging, and original.” But most reviews of it by psychologists—especially those taking some sort of behaviorist perspective—were extremely negative.⁶⁸ And Boring was one who decried what Köhler had done. He knew that the lectures would not focus on experimental studies and their implications, but apparently he had expected Köhler to talk about psychology, even if from a theoretical perspective. Instead, Köhler spoke on epistemology and metaphysics, and Boring was angry. At issue, too, was more than a simple tension between philosophy and psychology. In 1934, at least one psychologist wrote to Boring that the single lecture by Köhler that he had heard did not impress him, and Boring’s reply reveals much. “You commented on being disappointed in a lecture which you heard recently. I can say only that I heard the whole series and am terribly disappointed, and a little humiliated at the knowledge that I took the time to go to them. The content was not well informed nor related to current knowledge. The ideas were not important or clear. Most of the argument was childishly elementary, although I caught suggestions of something sinister behind the scenes once in a while—but I was never sure. The vocal presentation was dull and tiresome, although the literary was, if you could grasp it, exceptionally able. This then is what we applaud so heartily!”⁶⁹

To be sure, Boring was at a critical point in his life and about to undergo psychoanalysis in an attempt to free himself from the despondency that plagued him throughout the mid-1930s.⁷⁰ But his criticisms of Köhler were as much intellectual as personal. Apparently, soon after Köhler completed his William James lectures, the philosophers at Harvard urged that he be appointed professor at the university, and, with the situation in Germany worsening, Köhler was more open to such an appointment than ever before. But Boring, as director of the psychological laboratory and head of the Department of Psychology within the Division of

⁶⁷ Boring to Köhler, March 19, October 2, November 7, 1934; Boring to Perry, April 13, 1934, Boring Papers; and Boring to Jean and George Mandler, February 14, 1968, Mandler Papers.

⁶⁸ See *Journal of Philosophy*, 36 (1939): 107–08; *New York Times*, February 5, 1939, p. 6; Harry L. Hollingworth, review of Köhler’s *Place of Value in a World of Facts*, in *American Journal of Psychology*, 53 (1940): 146–52; and J. R. Kantor, review of Köhler’s *Place of Value in a World of Facts*, in *Psychological Bulletin*, 36 (1939): 292–96; and Köhler, *The Place of Value in a World of Facts* (New York, 1938).

⁶⁹ Boring to Leonard Carmichael, [December 20, 1934]; Carmichael to Boring, December 15, December 22, 1934, American Philosophical Society Library, Philadelphia, Leonard Carmichael Papers.

⁷⁰ Boring, *Psychologist at Large*, 53–54; and Jaynes, “Edwin Garrigues Boring,” 107. Also see Boring and Hanns Sachs, “Was This Analysis a Success?” *Journal of Abnormal and Social Psychology*, 35 (1940): 4–16.

Philosophy and Psychology, adamantly opposed the appointment. This position also gave Boring easier access to James B. Conant, a chemist who was Harvard's new president, and he was, at last, able to convince the Harvard administration to close the door permanently to Köhler. In all, Boring's opposition was final, and in 1935 he was able to appoint Lashley to a Harvard professorship, thus bringing to Cambridge the type of experimentalist he always wanted. (An earlier offer to Tolman fell through, in part because the Californian "love[d] Berkeley and dislike[d] Cambridge.")⁷¹ Despite this success, Boring's despondency grew, as his role in this incident apparently cost him several friends at Harvard. Even in the 1980s rumors still circulate about the feud between Boring and Köhler, and how it cost Köhler a Harvard professorship.⁷² In any event, by the end of 1935 Köhler had settled at Swarthmore College, where he established an institutional base for his work comparable to Koffka's at Smith. There he was a major influence on the development of such distinguished psychologists as Mary Henle, Solomon Asch, and Robert B. MacLeod. Within a year or two Boring and Köhler were again corresponding cordially, and after World War II Boring readily admitted Köhler's great influence on American psychology. In the 1950s Köhler was honored by election to the National Academy of Sciences—in fact, he was elected as soon as he became eligible through naturalization⁷³—and to the presidency of the American Psychological Association.

THE TRANSMISSION OF GESTALT PSYCHOLOGY was a complicated affair, but several points are especially striking. One major conclusion is that, like the transit of the new physics to America,⁷⁴ the diffusion of Gestalt psychology from Germany to the United States began long before Hitler came to power. Americans were too interested in German ideas and Germans too interested in American opportunities to wait until political events forced them into contact. By 1930 Gestalt psychology was firmly established in the United States as a psychological school, and graduate students interested in a Gestalt-focused education in psychology knew where to study. The rise of the Nazis certainly contributed to the completion of the migration, but it did not determine the direction in which American psychology developed. Another point worth noting is that Americans found that they had to react to at least three different factors—Gestalt psychology, the Gestalt movement, and the Gestalt psychologists themselves. They responded differently to each. Some, like Boring, were attracted by Gestalt experimental work but reacted negatively to Gestalt theory. And clearly, despite Koffka's disclaimer, the protagonists of Gestalt theory constituted a movement that tried to convert Americans to their view of the world. Americans quite naturally resented their missionary efforts.

⁷¹ Boring to Terman, March 27, 1928, Terman Papers; and Beach, "Karl Spencer Lashley."

⁷² Helson to Jean and George Mandler, February 27, 1968, Mandler Papers; Boring to Perry, November 28, 1933; Perry to Boring, October 28, December 1, 1933, Boring Papers; and Jaynes, "Edwin Garrigues Boring," 107.

⁷³ Boring to Terman, March 7, 1947, Terman Papers; and Boring to Jean and George Mandler, February 14, 1968, Mandler Papers.

⁷⁴ Stanley Coben, "The Scientific Establishment and the Transmission of Quantum Mechanics to the United States, 1919–32," *AHR*, 76 (1971): 442–66.

The personalities of the psychologists involved—both German and American—also helped shape the course of the migration. Certainly Köhler's formality, Boring's oversensitivity, and the concern for status that both men shared affected their interactions. And yet, despite the dominance of behaviorism, American psychology was open to Gestalt ideas, and today many Gestalt interpretations of psychological phenomena have joined the mainstream of modern American psychology. Certainly perception is an area of research molded by Gestalt ideas. Even in the 1920s and 1930s, the experimental work of leading American psychologists strongly exhibited the influence of the Gestalt approach to their science. Lashley's stress on basic neural mechanisms and, especially, Tolman's "Purposive Behaviorism"—one of the most influential approaches to psychology in the 1930s—clearly owed much to Gestalt theory. That both were offered positions at Harvard at a time when Köhler's suitability for such a position was being debated further illustrates how Americans differentiated between the psychologists and the ideas. In other areas of psychology, too, Gestalt concepts have been influential. Lewin's work, for example, with its stress on an individual's interaction with his or her "life space" or social field, has done much to shape modern social psychology.

Finally, and perhaps most controversially, I think we can conclude that the Gestalt psychologists themselves were well received in the United States. To be sure, the few who still define themselves as Gestalt psychologists argue that America did not give their teachers what it should have. But in many ways, this attitude is reminiscent of the situation of the 1920s and early 1930s when Koffka and Köhler allowed their movement to get the better of their ideas and preached to the Americans, trying to convert the heathen to the true gospel. In any event, all major Gestalt psychologists found positions in America in the middle of the depression and were able to carry on with the work they had started in Germany. This work enriched American psychology greatly and did much to counter the attractions of extreme behaviorism. If Gestalt psychology has today lost its identity as a school of thought—and very few of Koffka's, Köhler's, Wertheimer's, or Lewin's students call themselves Gestalt psychologists—it is not because the mainstream of American psychology has swamped their ideas. Rather, their work has done much to redirect this mainstream, which adopted many of their points of view. Few other migrating scientific schools have been as successful.⁷⁵

⁷⁵ Several distinguished scholars will disagree with most of these conclusions and have, in fact, drawn others directly opposed to them. For example, see Mary Henle, "The Influence of Gestalt Psychology in America," in R. W. Reiber and Kurt Salzinger, eds., *The Roots of American Psychology: Historical Influences and Implications for the Future* (New York, 1977), 3–12.

Barristers and Judges in Early Modern Barcelona: The Rise of a Legal Elite

JAMES S. AMELANG

IN A DESCRIPTION OF BARCELONA published in 1589, the antiquary Hieronim de Jorba remarked that “lawyers are in this city very principal persons.”¹ For once this garrulous pedant failed to exaggerate. Many barristers and judges distinguished themselves in Catalan political movements and institutions and as writers and intellectuals during the sixteenth and seventeenth centuries. It hardly seems an accident that Antoni Ferrer, the wily *uomo di carattere* of Manzoni’s *I Promessi Sposi*, was a native Catalan, who obtained his law degree from the University of Barcelona prior to entering the royal administration in Italy. His subsequent fame, while overshadowing that of less powerful contemporaries, symbolizes the prominence attained by many Catalan jurists during this era. Recent studies by William Bouwsma, Lauro Martines, Lawrence Stone, Wilfred Prest, Richard Kagan, and others have shown a marked increase in the size and prominence of the legal profession throughout Europe during the sixteenth and seventeenth centuries.² While advancing many explanations for this phenomenon, they have tended to focus on two as having the greatest influence: the emergence of the modern state, which provided new employment opportunities, and the general economic expansion of the late fifteenth and sixteenth centuries, which increased litigation and fostered a growing “market” for legal services. The city of Barcelona provides an arena in which to test these conclusions and to suggest additional reasons for this development.

To Jorba, Barcelona was “singularly illustrious, ancient, rich and powerful.” Although its population of thirty thousand to thirty-five thousand permanent

This essay incorporates portions of my doctoral dissertation, “Honored Citizens and Shameful Poor: Social and Cultural Change in Barcelona, 1510–1714” (Princeton University, 1981), which contains more extensive documentation of the arguments made here. I am deeply indebted to Cheryll Cody, Jonathan Dewald, Patrick Geary, Kermit Hall, Richard Kagan, and Theodore Rabb for their comments and criticism.

¹ Hieronim de Jorba, *Descripción de las excelencias de la muy insigne ciudad de Barcelona* (Barcelona, 1589), f. 29v.

² Bouwsma, “Lawyers and Early Modern Culture,” *AHR*, 78 (1973): 303–27; Martines, *Lawyers and Statecraft in Renaissance Florence* (Princeton, 1968); Stone, “The Educational Revolution in England, 1560–1640,” *Past and Present*, 28 (1964): 41–80; Prest, *The Inns of Court under Elizabeth I and the Early Stuarts, 1590–1640* (Totowa, N.J., 1972); Prest, ed., *Lawyers in Early Modern Europe and America* (New York, 1981); and Kagan, *Students and Society in Early Modern Spain* (Baltimore, 1974). Kagan’s *Lawsuits and Litigants in Castile, 1500–1700* (Chapel Hill, 1981) is the best study to date of the legal system of early modern Spain.

residents seems small in comparison with other contemporary Mediterranean cities, it clearly dominated the Catalan towns in size and importance. By the late Middle Ages, Barcelona was a leading Mediterranean center for cloth, silk, and ceramics production. It traded these wares as far as the Spanish Levant, Balearic Islands, Sardinia, Sicily, and southern Italy. Yet the city encountered increasing economic and social difficulties.³ Sharp losses in population combined with the faltering competitiveness of local products diminished its commercial and industrial strength. Disagreements over economic policies and recent political reforms finally gave way in the later fifteenth century to civil wars, which exacerbated the city's decline. A modest recuperation in the sixteenth century ended with the general Mediterranean trade crisis of the 1620s. Despite signs of recovery in the late seventeenth century, Barcelona's productive base slowly eroded under foreign competition. Notwithstanding Catalonia's relatively privileged position within the peninsular economy, the closing decades of the seventeenth century saw Genoese, French, and Dutch merchants increase their dominance over maritime commerce and the distribution of finished goods.

As Barcelona's trade and industry declined, its role as the political and administrative capital of the principality expanded. Located in the city were all three leading institutions of Catalan public life: the Barcelona city government; the *Generalitat*, or standing commission of the three estates in parliament; and the viceregal government, representing central monarchical authority. Thus, despite the comparatively reduced size of the government apparatus—constitutional restrictions limited the number of crown officials, in sharp contrast to the dramatic expansion of the royal bureaucracy in Castile—Barcelona possessed a marked administrative character. Beginning in the later sixteenth century, the growing pretensions of the central government led to clashes with the *Generalitat* and the city council. During the administration of the Count-Duke of Olivares (1621–43), the royal favorite, relations between the principality and the central government degenerated to the point of open rupture. The rising fiscal demands occasioned by the Thirty Years' War alienated practically all levels of Catalan society. Violent peasant uprisings in 1640 triggered a general revolt, which led to the principality's secession from the Hispanic monarchy and annexation by France. The reconquest of Barcelona by Castilian troops in 1652 ushered in the “neo-foral” epoch, marked by the political quiescence of the local ruling classes.⁴ Continued hostilities with France influenced the Catalans' decision in 1705 to support the Habsburg pretender to the Spanish throne. Defeat in the War of the Spanish Succession—which ended with the storming of Barcelona on September 11, 1714—constituted one of the decisive events in the history of the Catalan nation. The subsequent *Nova*

³ My account of the economic and political history of early modern Catalonia is based on Pierre Vilar, *La Catalogne dans l'Espagne moderne*, 1 (Paris, 1962), 461–710; and John H. Elliott, *The Revolt of the Catalans: A Study in the Decline of Spain, 1598–1640* (Cambridge, 1963).

⁴ The term “neo-foral” derives from the medieval *for* or *fur* (in Spanish, *fuero*), which refers to “privilege” or “franchise.” The Catalan historian Jaume Vicens i Vives coined it to designate those years of the later seventeenth century when the monarchy's control over the states of the Crown of Aragon slackened appreciably. Especially significant was the abrupt disappearance of the violent confrontations with local constitutional traditions so characteristic of the first half of the century. The most recent study of this era is Henry Kamen, *Spain in the Later Seventeenth Century, 1665–1700* (London, 1980).

Planta, or “new beginning,” ushered in a radically different era in Catalan politics, with the principality no longer existing as a separate political entity within the Hispanic monarchy. It was against this backdrop of economic stagnation and heightened political activity that Barcelona’s legal elite rose to power and high social standing.

THE EARLIEST REFERENCES TO LAWYERS IN BARCELONA date from royal privileges issued in the thirteenth century.⁵ Their wording reveals a growing division of labor within the practice of law. Barristers (*advocats* or *juristes*) occupied the upper reaches of the profession. These were jurists—doctors in law trained in civil or canon law or both at a recognized university—who rendered written opinions on matters of jurisprudence. Not all barristers became judges, but a doctorate in law was necessary for an appointment to the bench. Less prestigious and more numerous were the notaries and attorneys, known as *procuradors*, *causídics*, or *notaris*. They despatched the burgeoning amount of legal paperwork—recording depositions, entering cases into the docket, overseeing court appearances, as well as drafting standard “notarial” documents like wills and marriage contracts. The consolidation of a wide range of local administrative and judicial institutions led to growth in the size and prominence of all branches of the profession. By the end of the Middle Ages, Barcelona had clearly become a “legal city,” whose concentration of tribunals closely resembled contemporary *parlementaire* seats in France.⁶ The conversion in the late fifteenth century of the erratic royal council into the fixed *Audiencia* or appellate court helped channel demand for legal services throughout Catalonia toward the capital city. A host of minor tribunals surrounded the royal appeals court, including the crown’s fiscal tribunals, courts of first instance, and separate jurisdictions like the Merchant Consulate, whose decisions were not subject to appeal. Of these the *Audiencia* was clearly the most prestigious court for the city’s numerous judges.⁷

The absence of corporate organization at the higher levels of the profession also influenced local legal practice. In 1343 the city councilors abolished the guild of barristers founded a decade earlier. Surprisingly, the large profession failed to override this decision. In fact, prior to the foundation in 1777 of Barcelona’s Academy of Jurisprudence, barristers were the only major urban occupational group to lack a recognized guild or confraternity. One explanation for this striking anomaly within a vigorously corporatist society centers on the barristers’ desire to

⁵ Ferran Valls Taverner, “Los abogados en Cataluña durante la edad media,” in his *Obras*, volume 2: *Estudios histórico-jurídicos* (Madrid and Barcelona, 1954), 281. Despite the prominence of the legal profession in medieval and early modern Barcelona, little attention has been devoted to the social history of Catalan lawyers. This neglect contrasts with the many studies of the history of Catalan legislation and jurisprudence. See, for example, Guillem M. de Brocà, *Historia del derecho civil de Cataluña* (Barcelona, 1918); and Santiago Sobrequés i Vidal, *Història de la producció del dret català fins al Decret de la Nova Planta* (Girona, 1978).

⁶ Lenard Berlanstein used this term in his *The Barristers of Toulouse in the Eighteenth Century* (Baltimore, 1975), especially in chapter one. Other useful studies of *parlementaire* towns include William Doyle, *The Parlement of Bordeaux and the End of the Old Regime, 1771–90* (London, 1974), pt. 1; and Jonathan Dewald, *The Formation of a Provincial Nobility: The Magistrates of the Parlement of Rouen, 1499–1610* (Princeton, 1978).

⁷ A satisfactory institutional history of the Catalan *Audiencia* has yet to be written. For partial accounts of its development, see Pere Voltes Bou, “La Audiencia de Barcelona durante la Guerra de Sucesión,” *Revista Jurídica de Cataluña*, 68 (1962): 330–34; and *Guia abreviada al archivo de la Corona de Aragón* (Madrid, 1958), 59–68.

distinguish themselves socially from the lower ranks of the profession, already grouped into several corporate bodies.⁸ Entry into the legal elite also proved one of the more privileged avenues of upward social mobility. That this path was a well-traveled one owed a great deal to the absence of the corporate restrictions. The lack of regulation—the lone attempt by parliament to limit the number of advocates in the *Audiencia* failed in 1626—perpetuated the open character of the legal establishment.⁹

Thomas Platter's remark of 1599 that in Spain "barristers are less numerous than in France" is difficult to verify for Barcelona, given the haphazard preservation of local records.¹⁰ Only a few volumes of the bar register established in 1328 survive: 1551–73, 1606–53, and 1659–1703.¹¹ These rolls listed every recruit to the local bar, his academic title (bachelor, licenciado, or doctor in canons, civil law, or *utriusque iuris*), the university where he studied, and often his place of origin, father's name and profession, and social rank. The most significant expansion in the number of entering barristers apparently took place in the early seventeenth century, and during the decades immediately following the war of 1640–52 (see Graph 1).

Other sources enable a reconstruction of the total number of barristers in Barcelona. Diaries and the list of competitors for a vacant chair in civil law at the university suggest a minimum of one hundred thirty barristers during the later sixteenth and early seventeenth centuries.¹² In 1716 the incoming Bourbon administration ordered all practicing doctors in law to sign a new registry, thus incorporating under direct royal control the jurists hitherto supervised by the city councilors. One hundred sixty-five barristers signed the list—quite a large number, considering the recent wars and the numerous jurists exiled as partisans of the Habsburg pretender.¹³ Sources for the later years of the century report approximately two hundred barristers practicing in the city.¹⁴

Clearly the number of barristers relative to the estimated urban population reached its zenith during the late seventeenth and early eighteenth centuries. Subsequently the legal profession failed to keep pace with demographic growth, which accelerated during the late eighteenth century (see Table 1).¹⁵ The highest

⁸ Philip Benedict noted this tendency in sixteenth-century Rouen; see his *Rouen during the Wars of Religion* (Cambridge, 1981), 8–9. The Iberian peninsula in general apparently lagged behind the rest of Europe in the establishment of collegiate bodies of jurists.

⁹ Lluís de València, *Ilustración a la Constitución VII* (Barcelona, 1674), 49.

¹⁰ Platter, *Journal of a Younger Brother*, trans. Sean Jennett (London, 1963), 228.

¹¹ The surviving rolls of the *Matrícula dels jurisperits* can be found in the Arxiu Històric Municipal de Barcelona, Barcelona [hereafter, AHMB], Vegueria, series 15, in five volumes. Although few records have survived from the Universities of Barcelona and Lleida, where most of these barristers studied, I have been able to identify over thirteen hundred men who received degrees or practiced law in Barcelona from 1600 to 1714.

¹² Jorba, *Descripción*, ff. 25v, 29v; and Jeroni Saconomina, *Llibre de memòries* (1576–1603), in AHMB, MS. B-29, f. 86r. The list of law graduates competing in the *oposicions* of 1600 can be found in AHMB, Consell de Cent XVIII, "Estudi General," volume 9. These lists excluded the judges of the *Audiencia*.

¹³ *Matrícula de los abogados de la real Audiencia de Cataluña*, Arxiu de la Corona d' Aragó, Barcelona [hereafter, ACA], Real Audiencia, varia 79. This volume covers 1716 to 1742; varia 4, 1758 to 1771. For a partial list of exiles, see Fèlix Duran i Canyameras, *Els Exiliats de la Guerra de Successió* (Barcelona, 1964).

¹⁴ Figures for the later eighteenth century can be found in the *Guías de forasteros* published on an irregular basis beginning in 1777. The figures for 1849 derive from Manuel Saurí and José Matas, *Guía general de Barcelona* (Barcelona, 1849), 66, 292–97.

¹⁵ For the demography of Barcelona during these years, see Jordi Nadal and Emili Giralt, *La población catalana de 1553 a 1717* (Paris, 1960), 202; Nadal, "Le mouvement démographique de la Catalogne de 1787 à 1815," paper presented at the Twelfth International Congress of Historical Sciences, held in Vienna, 1965;

ratio of barristers to urban population occurred in the “neo-foral” period prior to the consolidation of Bourbon rule. The large size, in both absolute and relative terms, of Barcelona’s legal profession takes on even sharper relief when contrasted with the number of lawyers in other Iberian cities. The census of the 1560s and other sources provide estimates for cities in New Castile (see Table 2). Although the comparison is far from exact, given discrepancies in dates, types of sources used, and the different categories employed in each survey, these data illustrate, nevertheless, the large size of the legal establishment in Barcelona. Especially striking is the contrast in the number of barristers. Despite notable similarities between Barcelona and Valladolid (both contained some thirty thousand to forty thousand inhabitants and housed the most active *Audiencias* of their respective crowns), the number of barristers in Barcelona was more than double that of its Castilian counterpart. In short, Barcelona possessed, at least by early modern Spanish standards, an unusually large group of doctors in law. The public prominence of local barristers, however, did not rest exclusively on the size of this group. The legal community also derived influence and authority from membership in the city’s ruling class.

The number of barristers and judges admitted during the sixteenth and seventeenth centuries to the municipal elite of *ciutadans honrats* provides an index of the growing civic protagonism of the legal profession. These “honored citizens” were wealthy rentiers of recent merchant or professional background.¹⁶ In 1510 King Ferdinand the Catholic conferred noble privileges on the group, while limiting its initial membership to a fixed *matrícula* or registry. Over one-half of all new matriculants during the years 1510 to 1714 held advanced degrees in law—a remarkably high proportion considering the diverse social and occupational origins of the citizen oligarchy, which found recruits among merchants, physicians, notaries, government officials, and upper guild masters.¹⁷ The presence of barristers and judges among both old and new honored citizens gives an even clearer picture of the rise of jurists within the oligarchy (see Graph 2). The royal privilege of 1510 included some fifteen barristers (13.7 percent) among the 109 individuals specifically named as honored citizens. This proportion increased steadily beginning in the mid-sixteenth century, reaching its climax during the final decades of elections when one-half of all *ciutadans de matrícula* were doctors in law, and at all times they constituted the single largest occupational group within the body of matriculants.

Participation in municipal government also illustrates the growing influence of jurists in Barcelona’s politics. Royal policies permitting barristers to occupy lucrative and prestigious positions reflected their increasing prominence within local society. In 1455 King Alphonso the Magnanimous agreed to a significant restructuring of

Vilar, “Un moment crític en el creixement de Barcelona 1774–1787,” in Vilar, *Assaigs sobre la Catalunya del s. XVIII* (Barcelona, 1973), 43–55; and Laureano Figuerola, *Estadística de Barcelona en 1849* (Madrid, 1968), 34–39.

¹⁶ For a detailed study of the citizen oligarchy, see Amelang, “Honored Citizens and Shameful Poor: Social and Cultural Change in Barcelona, 1510–1714” (Ph.D. dissertation, Princeton University, 1981), chaps. 1–4.

¹⁷ *Matrícula dels ciutadans de Barcelona*, AHMB, MSS. L-56, L-57.

TABLE 1
Ratio of Barristers to Total Urban Population of Barcelona, 1389–1849

Year	Total Number of Barristers	Approximate Urban Population	Ratio of Barristers per 1000/Population
1389	39	40,000	0.9
1589	130	35,000	3.7
1600	140	37,500	3.7
1716	165	30,000	5.5
1798	200	100,000	2.0
1806	215	100,000	2.1
1815	128	50,000	2.5
1849	369	165,000	2.2

SOURCES: Martí de Riquer *et al.*, *Onomástica barcelonesa del s. 14* (Barcelona, 1977); Arxiu Històric Municipal de Barcelona, Vegueria, ser. 15; Hieronim de Jorba, *Descripción de las excelencias de la muy insigne ciudad de Barcelona* (Barcelona, 1589); Arxiu Històric Municipal de Barcelona, Consell de Cent XVIII, "Estudi General," vol. 9; *Matricula de los abogados de la real Audiencia de Cataluña*, Arxiu de la Corona d'Aragó, Real Audiencia, varia 79; *Calendario manual y guía de forasteros en Barcelona* (Barcelona, 1798); Manuel Saurí and José Matas, *Guía general de Barcelona* (Barcelona, 1849); and Jordi Nadal and Emili Giralt, *La population catalane de 1553 à 1717* (Paris, 1960).

the two major organs of the Barcelona government: the five councilorships and the plenary assembly known as the *Consell de Cent*, or "Council of the Hundred." Beginning in that year, *gaudints* (literally, "those who enjoy" doctorates in law or medicine) vied for positions formerly reserved exclusively for honored citizens.¹⁸ Only a handful of barristers and physicians had occupied councilorships from their establishment in the mid-thirteenth century to the late fifteenth century,¹⁹ but this situation began to change during the sixteenth century (see Graph 3). Beginning in the 1550s, the number of jurists and physicians serving as councilors increased steadily before reaching a plateau of 35 percent to 45 percent between 1610 and 1680. During the final decades of this regime (1690–1714), their numbers grew to record proportions. As an anonymous chronicler of the late seventeenth century complained, members of the liberal professions "have multiplied so much that today they fill almost all these posts."²⁰

Did the improved fortunes of the legal profession in sixteenth- and seventeenth-century Barcelona reflect the absorption of a new social group into the ruling class

¹⁸ Carme Batlle i Gallart, *La Crisis social y económica de Barcelona a mediados del s. XV*, 2 (Barcelona, 1973), 466–75; and Jaume Vicens i Vives, *Ferran II i la ciutat de Barcelona* (Barcelona, 1936), esp. vol. 1. Alphonso's administrative reforms enhanced the status of jurists in other states of the Crown of Aragon; see Alan Ryder, *The Kingdom of Naples under Alfonso the Magnanimous: The Making of a Modern State* (Oxford, 1976), 370.

¹⁹ Few barristers or physicians served as councilors from 1250 to 1350. The later fourteenth century saw a temporary change; thirty-six of approximately four hundred councilors from 1345 to 1424 were jurists. Their participation suddenly receded around 1420; only six barristers and one "master in medicine" occupied posts from 1425 to 1514. For a discussion of the methods used to determine the names and occupations of the councilors, see Amelang, "Honored Citizens," 128.

²⁰ AHMB, MS. B-44, f. 34v. The plenary Council of the Hundred also experienced a similar rise in the proportion of barristers and physicians. For a detailed breakdown of its membership during the sixteenth and seventeenth centuries, see Amelang, "Honored Citizens," 96–97, 144–47.

TABLE 2
Number of Lawyers per Thousand Population in Early Modern Spain

	Medina del				
	Barcelona	Segovia	Valladolid	Campo	León
Urban	ca. 1590	1561	1589	1561	1561
Population	37,000	17,500	32,500	16,500	10,000
Castelló					1608
					8,000
ATTORNEYS:					
Notarios y					
Escribanos	2.1	2.4	3.5	1.5	
Procuradores	—	1.4	0.5	0.7	
Solicitadores					
y Causídicos	2.0	0.1	0.9	—	
Subtotal	4.1	3.9	4.9	2.2	1.2
BARRISTERS	3.4	0.6	1.0	0.4	2.0
(Abogados)					
TOTAL	7.5	4.5	5.9	2.6	3.2
					4.2

NOTE: The generous coefficient of 5.5 inhabitants per *vecino* (hearth) has been used to arrive at the following approximate figures for total urban population. The Toledo estimates conflate "Procuradores, escribanos y notarios" in what seems to be an artificially low figure. On the other hand, the number of barristers is probably overestimated, as this category includes "doctores, maestros, licenciados y bachilleres," many of whom obviously did not practice law.

SOURCES: Castile—Bartholomé Bennassar, "Économie et société à Ségovie au milieu du 16^e s.," *Anuario de historia económica y social*, 1 (1968): 202; "Medina del Campo: Un exemple des structures urbaines de l'Espagne au 16^e s.," *Revue d'histoire économique et sociale*, 39 (1961): 493; Richard Kagan, *Lawuits and Litigants in Castile, 1560-1700* (Chapel Hill, 1981), 63; Linda Martz and Julio Porres, *Toledo y los toledanos en 1561* (Toledo, 1974), 305-07; and Valentina Fernández Vargas, *La población de León en el s. XVI* (Madrid, 1968), 145. Castelló—James Casey, *The Kingdom of Valencia in the Seventeenth Century* (Cambridge, 1979), 46. Barcelona—Hieronym de Jorba, *Descripción*, ff. 25v. and 29v.; Josep M. Madurell i Marimon, *Índice cronológico alfabético del Archivo General de Protocolos de Barcelona*, 2 (Barcelona, 1953), 347-50; and the *Taula dels causídichs matriculats* (Barcelona, 1593).

TABLE 3
Court Fees in the *Audiencia* of Barcelona, 1528–1690

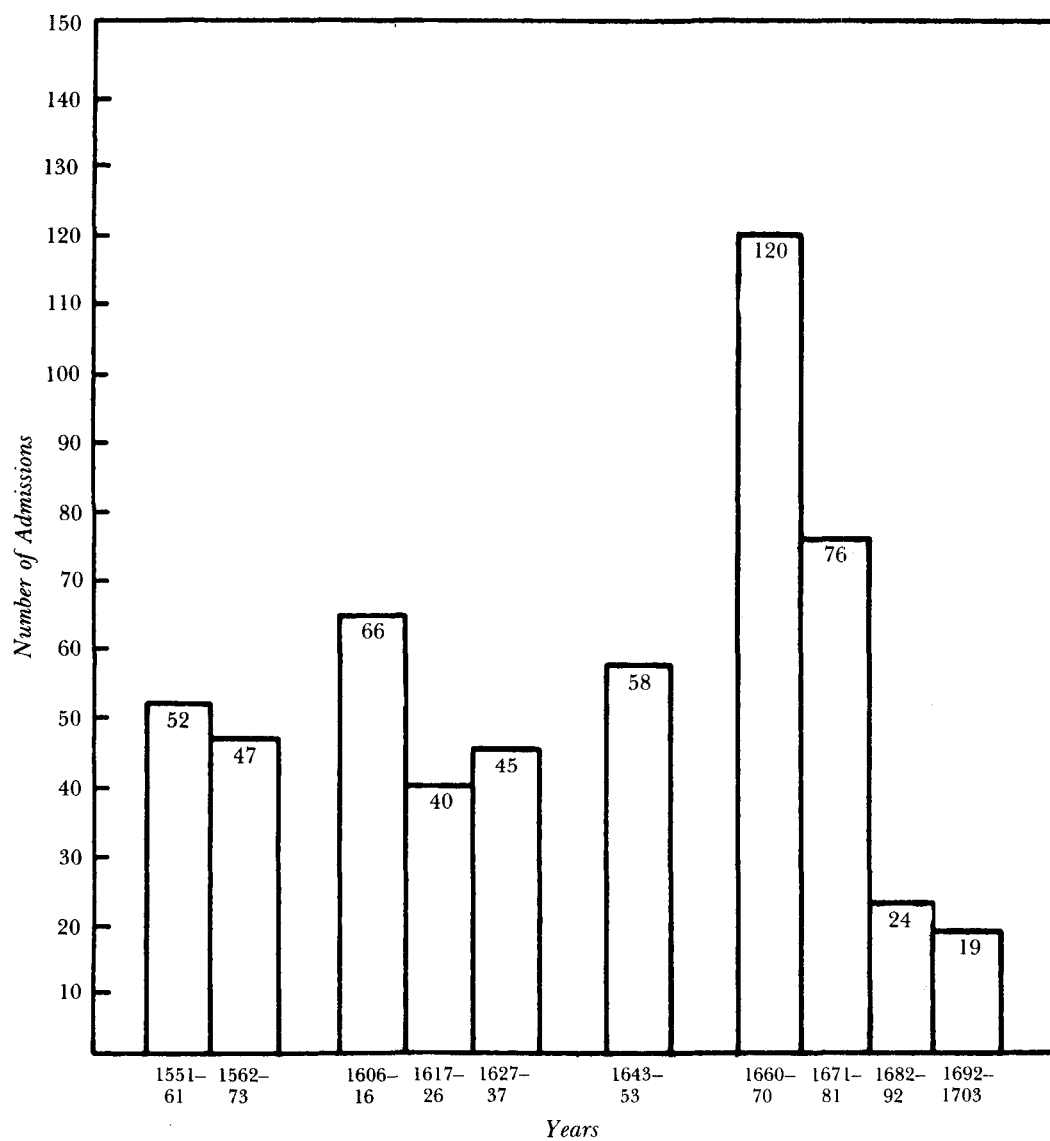
<i>Year</i>	<i>Total Number of Cases</i>	<i>Total Fees Charged</i>	<i>Fee per Case</i>
1528	608	£1,126	£1 10s.
1531	836	1,947	2 4
1555	1,280	2,884	2 4
1556	1,153	3,131	2 9
1580	1,089	3,563	3 3
1581	1,079	4,109	3 10
1597	1,433	9,082	4 4
1603	1,593	6,866	6 4
1630	1,290	7,315	5 8
1631	1,239	8,449	6 10
1657	594	6,056	10 2
1658	657	8,237	12 6
1674	1,195	10,494	8 9
1675	1,072	9,399	8 9
1689	1,498	12,295	8 3
1690	1,512	12,503	8 3

SOURCE: Arxiu de la Corona d'Aragó, Generalitat, ser. G-60, vols. 1–32 (1513–1691).

or changing occupational predilections within this group? Apparently both tendencies were present, which suggests a more general transformation of the socio-economic foundations of the urban elite. The liberal professions offered significant opportunities for social mobility. Entry into the urban ruling class came by stages, the first being *gaudint*. The next stage in the slow upward climb was elevation to the rank of honored citizen. And yet the achievement of this status did not necessarily guarantee the continued practice of law within elite families. The proportion of sons of citizen jurists who also became barristers was fairly low, averaging 38 percent for the entire period.²¹ Extension of this analysis from two generations to three makes it immediately evident that the chance of maintaining a legal practice from grandfather to grandson within an honored citizen family was quite slim indeed. Ironically, the “deprofessionalization” of successful barrister families was accompanied by a significant, if somewhat less striking, reorientation of existing elite families toward the study and practice of law. Numerous noble families contributed sons to the legal profession. Nor were these sons just cadets to be fobbed off on the local university as on the church. For example, Don Ramon Dalmau de Rocabertí—eldest son and heir to the viscount of Rocabertí, head of a leading family in the Catalan peerage—proudly referred to himself as a “graduate

²¹ For a statistical summary of the continuity of the practice of law within honored citizen families, see Amelang, “Honored Citizens,” 123–24, 146–47.

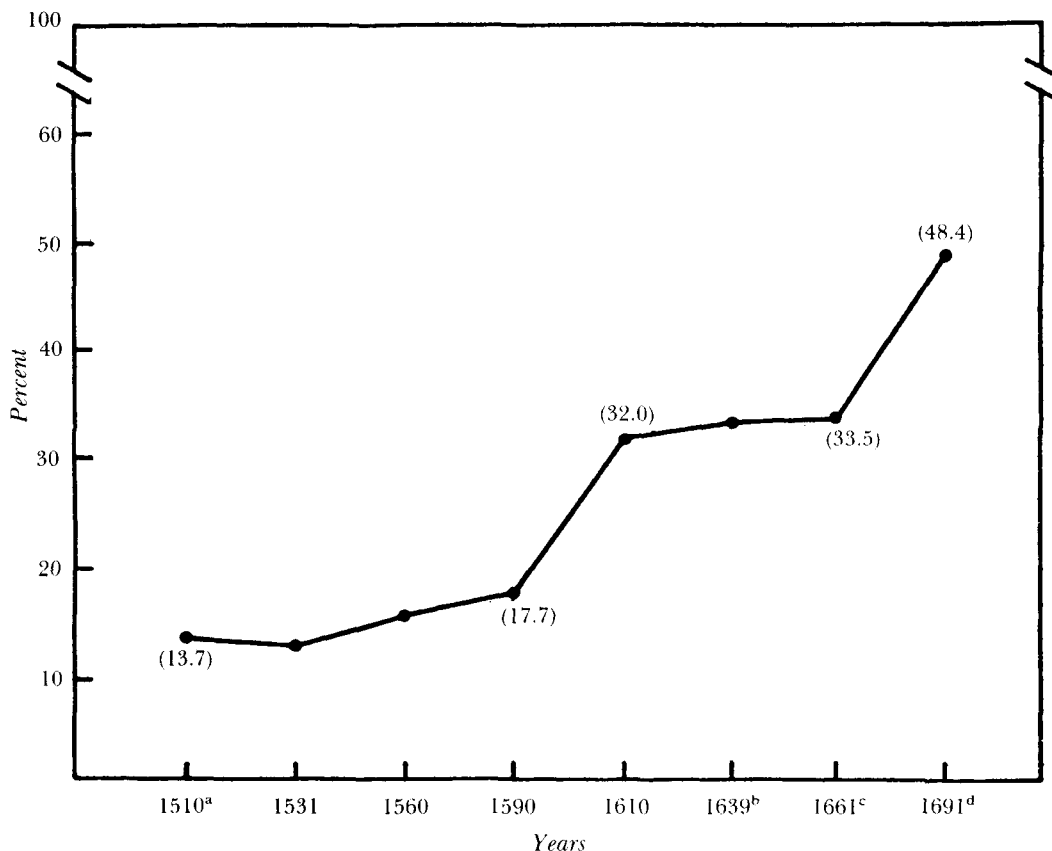
GRAPH 1
Number of Admissions to Barrister Registry, 1551–1703



SOURCE: *Matrícula dels Jurisperits*, Arxiu Històric Municipal de Barcelona, Vegueria, ser. 15.

GRAPH 2

Percentage of Jurists in Attendance at Assemblies of Honored Citizens, 1510–1691



^a An approximation of the number of honored citizens in 1510, compiled from the individuals listed in the original privilege, was used to calculate this percentage.

^b There was no meeting held in 1640. The next assembly was on May 1, 1642.

^c There is no record of attendance for the meeting of 1660.

^d There was no meeting in 1690.

SOURCE: *Matrícula dels ciutadans de Barcelona*, Arxiu Històric Municipal de Barcelona, MSS. L-56, L-57.

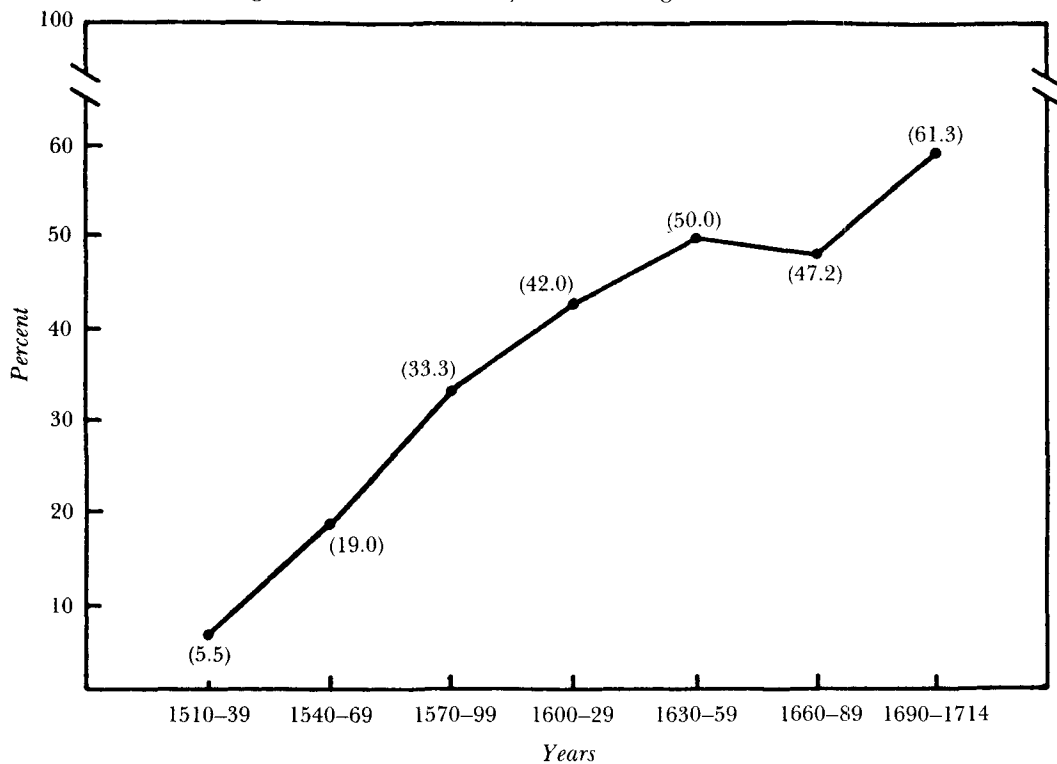
in both laws” on the title page of his political treatise, *Fatal Omens of French Rule in Catalonia*, published in 1646.²² In fact, by 1702 the number of noble law graduates had increased to such a point that parliament petitioned the king to award them precedence in consideration for vacant judgeships.

The specific case of the *Audiencia* or appellate judges illustrates the changing balance between these two trends. The magistrates of the sixteenth and early seventeenth centuries were essentially “new men” recruited from legal or merchant families. Only one noble—the famed jurisconsult Don Lluís de Paguera—served on the *Audiencia* prior to the 1630s. By 1640, four of the sixteen jurists were nobles, although this proportion suffered a sharp decline during the following decade until only one noble remained on the court. The Castilian restoration of 1652 brought in its wake a radical aristocratization of the bench. In the reconstituted chamber of

²² Rocabertí, *Presagios fatales del mando francés en Cataluña* (Saragossa, 1646). The term “both laws” refers to canon and civil law.

GRAPH 3

Percentage of Barristers and Physicians among Councilors, 1510–1714



SOURCE: Esteve Gilabert Bruniquer, *Rúbricas: Ceremonial dels magnífics consellers y regiment de la ciutat de Barcelona*, 1 (Barcelona, 1912); and James S. Amelang, "Honored Citizens and Shameful Poor: Social and Cultural Change in Barcelona, 1518–1714" (Ph.D. dissertation, Princeton University, 1981), 128.

1653, six of eleven judges were nobles; by the 1690s, nine of fourteen. Hence, by the end of the early modern period the predominance of honored citizens within the most prominent local tribunal—in 1602 all but five of the seventeen judges were *ciudadans de matrícula*—had been replaced by an equally imposing proportion of aristocrats.²³ Moreover, noble barristers rarely passed on their occupation to their sons. Only two judges of the largely aristocratic *Audiencia* of 1690 had barrister sons, compared with six in 1602.

SEVERAL SPECIFIC CAUSES underlay the dramatic improvement in the fortunes of Barcelona's legal elite. As in other European cities, the establishment of a variety of governmental and judicial institutions decisively encouraged growth in the number and social standing of local jurists. The *Audiencia* played a special role in fostering this trend. In sharp contrast to most of the peninsula, judges of the appeals court of Barcelona were without exception recruited among local barristers.²⁴ Catalan

²³ James Casey noted the identical tendency in the *Audiencia* of Valencia beginning in the 1630s; see his *The Kingdom of Valencia in the Seventeenth Century* (Cambridge, 1979), 191.

²⁴ There is no adequate study of the recruitment of *Audiencia* judges in the rest of Spain during the sixteenth and seventeenth centuries, although J.-M. Pelorson's *Juristes castillans sous Philippe III* (Le Puy, 1980)

constitutional law required that all royal officials except the viceroy be native Catalans. Moreover, the overriding necessity of familiarity with the special body of Catalan civil law and jurisprudence forced the crown to appoint candidates who met these requirements.²⁵ The fact that the highest ranks of this branch of the Spanish legal profession were not only open to but also reserved exclusively for members of the local legal establishment clearly enhanced job opportunities for the city's lawyers.

Available posts were hardly limited to those of prosecutors and judges of the *Audiencia*. The strength of Barcelona's municipal government and Catalan constitutional bodies like the *Generalitat* provided a number of key positions for legal advisers. For example, the *Generalitat* maintained a staff of two barristers, one prosecuting attorney, and many notaries and scribes. The city government also hired two permanent advisers and contracted outside legal assistance on a piecework basis. Other institutional clients—including the major monasteries and religious orders, the noble estate in parliament, many guilds, and all the principal aristocratic families—kept lawyers on retainer. There was, nevertheless, little increase in the number of formal administrative positions open to jurists during the sixteenth and seventeenth centuries. The addition in 1585 to the *Audiencia* of a criminal chamber composed of four new judges was the only major expansion within the local bureaucracy.²⁶ The burgeoning demand for legal services, which stimulated the rapid growth in the number of lawyers beginning in the mid-sixteenth century, reflected changing trends within already established institutions.²⁷

A remarkable increase in litigation accounts for this rise in demand. Despite repeated condemnations of the "litigious spirit" by preachers and other moralists, the proverbially quarrelsome Catalans resorted in growing numbers to the judicial system to settle their disputes. A sample of *Audiencia* civil cases and fees assessed as court costs reveals the general inflation within both categories (see Table 3).²⁸

contains some interesting information. For a brief examination of the social and geographical background of the magistrates of the Chancery of Valladolid, see Kagan, *Lawsuits and Litigants*, 175–89; and Pere Molas i Ribalta, "La Chancillería de Valladolid en el s. XVIII: Apunte sociológico," in Molas i Ribalta, ed., *Historia social de la administración española: Estudios sobre los siglos XVII y XVIII* (Barcelona, 1980), 183–246. On the *Audiencia* of Valencia, see Casey, *The Kingdom of Valencia*, chap. 8; and Pere Molas i Ribalta, "Los colegiales mayores en la Audiencia de Valencia, ss. XVII-XVIII," *Pedralbes*, 1 (1981): 51–75.

²⁵ A dramatic change in the recruitment of judges followed the abolition of the constitutional regime in 1714. From 1716 to 1818, most of the *Audiencia* magistrates, and all but one of its regents, were recruited from outside Catalonia. See Pere Molas i Ribalta, "Las Audiencias Borbónicas en la Corona de Aragón," in Molas, *Historia social de la administración española*, 153–57.

²⁶ *Constituciones y altres drets de Cathalunya* (Barcelona, 1704), 84. Details are also provided in the diary of the contemporary lawyer Perot de Vilanova, *Memórias per a sempre*, Biblioteca de Catalunya, Barcelona, [hereafter, BC], MS. 501, f. 162v.

²⁷ Virtually no work has been done on the largest paid bureaucracy in Catalonia, the church. Ecclesiastical legal proceedings apparently made up an important part of the practice of Barcelona lawyers, as evidenced by the presence of many *doctors en canons* within the city's legal elite.

²⁸ This sample of the case work of the *Audiencia* derives from the receipts for payment of legal fees deposited with the comptroller of the *Generalitat*: see ACA, Generalitat, ser. G-60, vols. 1–38 (1513–1691). The figures for the number of cases in Table 3 represent the total number of final sentences issued. Fees were calculated at a fixed percentage of the amount awarded victorious plaintiffs in civil cases; the fee schedule, established in 1503, did not alter during the period under study. The steady increase in the amount of fees can be attributed to the inflation of the "long century," ca. 1520–1650. The high mark of fees collected coincided exactly with the

These figures show a sustained rise in the number and value of cases throughout the early 1500s, which reached a climax at the turn of the century. A sharp drop in the wake of the revolutionary years, 1640–52, followed the slight decline evident by the 1630s. By the 1670s, however, the level of casework underwent a strong surge that continued to the end of the century.

Several other indicators point to a marked rise in litigation during the early modern period. Much of the legislation passed by the parliaments of the later sixteenth century (1564, 1585, and 1599) dealt with the growing backlog of cases within the *Audiencia*, which threatened the speedy rendering of justice. Parliament took major steps to bridge this impasse, including the streamlining of the appeals process by removing petty pleas from appellate jurisdiction. Perhaps the most forceful measure was the creation in 1585 of a third chamber dedicated exclusively to criminal affairs and to appeals from the first two civil *salas*. Whether or not these reforms solved the problem is open to doubt. Like most other courts, irritating delays and an unseemly backlog of unheard cases still plagued the *Audiencia* of the seventeenth century.

Extensive research has turned up only one sustained discussion of changing patterns of litigation in early modern Barcelona. In 1690 local attorneys protested a viceregal ruling allowing impoverished barristers to enter the attorneys' guild without submitting to the traditional public examination. The decree stated that possession of doctorates in law conferred upon these jurists the status of "poor gentlemen." The attorneys clearly resented the possibility of encroachment on their habitual preserves and argued that admission of barristers "would totally ruin us. For the endless number of doctors in law that abound in this city has, thanks to the great decline in the number of suits, solicited admission into the guild of attorneys, whose seventy members can barely sustain their own homes and families through the little they earn in the trade of despatching casework."²⁹ Despite self-serving exaggeration by the attorneys, this intriguing brief shows that the declining number of suits during the mid-seventeenth century left behind an excess population of barristers. Still, by the end of the century market prospects for all lawyers apparently improved to some degree, as evidenced by the upswing registered by 1690.

"Legal knowledge is an abundant treasure," the attorneys claimed. The size and prominence of the local bar suggests that the practice of law was indeed lucrative. Unfortunately, few early modern jurists aided historians by documenting their participation in the treasure hunt.³⁰ The diaries and private papers of seventeenth-century barristers are frustratingly silent on this and other aspects of their

decade of the most violent price rise in Catalonia (ca. 1640–55). Although there is no general price index for the principality similar to Earl Hamilton's work for Castile and Valencia, preliminary figures for this period can be found in Vilar, *La Catalogne dans l'Espagne moderne*, 633–35. I am grateful to Xavier Gil Pujol for his assistance in obtaining many of these figures.

²⁹ *Exposición del Colegio de Notarios Reales y Causídicos de Barcelona al virrey* (Barcelona, n.d. [1690]), in BC, Follets Bonsoms 400, p. 19.

³⁰ For studies of legal income in early modern Europe, see Wilfred Prest, "Counsellors' Fees and Earnings in the Age of Sir Edward Coke," in J. H. Baker, ed., *Legal Records and the Historian* (London, 1978), 165–84; and Martines, *Lawyers and Statecraft*, 100–06. For isolated references to the income of judges and barristers in early

professional lives. The only explicit reference to income occurs in a chance remark from around 1605, which suggests that a successful barrister could hope to make over £1,000 per annum in private practice.³¹ Furthermore, both the large size of the dowries given by jurists to their daughters and the property assessments in tax lists and registries of confiscations made after the siege of 1714 indicate that barristers and judges were often among the more wealthy members of the community.³²

This impression grows when one focuses on the most prominent members of the city's legal community, the judges of the *Audiencia*. In 1585 parliament fixed judicial salaries at the following levels: presidents of the first two chambers, £600–£650 per annum; "ordinary" or civil judges, £400; and criminal judges attached to the third chamber, £500. Beginning in 1493, however, fees assessed in proportion to settlements awarded in civil cases supplemented the salaries of the ordinary judges. These earnings soon constituted the major portion of the judges' total official income. By the early seventeenth century, a magistrate received between £600 and £900 in fees each year.³³ Moreover, the official salary of about £1,000–2,000 per annum³⁴ by no means constituted the only source of judicial revenue. Contemporary observers from all stations of life lambasted the corruption and dishonesty of the local bench. Parliamentary records document repeated attempts to stop judicial abuses, which ranged from the magistrates' habit of buying up land under litigation after fixing cheap prices through court orders to the acceptance of bribes in filling vacant posts on the bench. Other forms of corruption included the use of office to rig judgments in favor of friends and clients. For example, Ramon Rubí, who by his own account owed his post on the court to the patronage of the illustrious Cardona family, went out of his way to ensure the court's favorable treatment of the family's claims.³⁵ By 1670 this practice was so widespread that the noble estate was moved to protest against *Audiencia* judges serving as "proctors or administrators for grantees, titled nobles, and other private persons."³⁶

A rich array of satirical proverbs and other forms of popular criticism assailed the greed and corruption of the legal elite.³⁷ The confidential remarks of local jurists themselves made clear that such attacks did not err in their description of venal

modern Valladolid, see Bartholomé Bennassar, *Valladolid au siècle d'or* (Paris and the Hague, 1967), 362–72; and Kagan, "Lawyers and Litigation in Castile, 1500–1700," in Prest, *Lawyers in Early Modern Europe and America*, 187. Although Berlanstein included some interesting information on the fortunes of lawyers in the second chapter of his *Barristers of Toulouse*, he rarely mentioned proceeds derived specifically from legal practice.

³¹ Joan Magarola, as cited in Elliott, *Revolt of the Catalans*, 88, 559. One Barcelona pound equalled ten Castilian *reales*.

³² It is impossible to distinguish within these documents income derived exclusively from legal practice from other, nonprofessional sources of wealth, such as returns on real estate or money lending. Calculating income from legal practice not only reveals the lucrative possibilities of this profession but also establishes minimum figures for the overall earnings of lawyers.

³³ See the account books of the *Audiencia* in ACA, Generalitat, ser. G-60.

³⁴ Elliott also cited this sum; *Revolt of the Catalans*, 88. James Casey estimated the maximum income of a judge of the *Audiencia* in neighboring Valencia at £1,200; *Kingdom of Valencia*, 193–94.

³⁵ Rubí, *Relación del levantamiento de Cataluña*, Bodleian Library, Oxford. MS. Add. A-137, f. 117v.

³⁶ *Dietari del bras militar*, ACA, Generalitat, ser. G-68, vol. 3, May 9, 1671.

³⁷ For a collection of such proverbs, see Joan Amades, *Justicia popular* (Barcelona, 1935), s.v. "La llei." For other examples of literary attacks on lawyers, see Amelang, "Honored Citizens," 109–10.

practices within the profession. The diary of the barrister Jeroni Pujades (1600–35) painted a remarkably sordid picture of corruption and favoritism among contemporary *Audiencia* judges. In a similar vein, the secret evaluation drawn up in 1654 by the retiring judge Jaume Mir contained scathing references to the dishonesty and incompetence of his fellows on the bench. In light of such criticism, it is interesting to note the attitude of the tanner Miquel Parets toward the mob lynching of five magistrates in 1640. Although the author of this fascinating personal chronicle rejected popular violence, he repeatedly confirmed the charges of corruption and profiteering made against each of the murdered judges.³⁸

An attempt was made to combat judicial favoritism and dishonesty by establishing in 1599 inspections of the appellate court every six years. Parliament frequently demanded these tours as a means of controlling misconduct on the bench, but they proved incapable of ensuring lasting reform. The most they did was fine or temporarily suspend individual judges, as in 1565 and 1602.³⁹ Despite frequent and often bitter complaints, litigants had little choice but to follow the example of the barrister Josep Mora i Pons who carefully noted in his accounts the fifty-two partridges and four jugs of honey that he distributed every year among the thirteen judges of the *Audiencia*.⁴⁰ This resigned attitude in the face of widespread corruption found eloquent depiction in a 1636 sermon by the Augustinian friar Gaspar Sala who, in a desperate attempt to raise money for a new hospice for the poor, noted with disarming frankness that “when we wish to obtain a sentence in our favor from a judge here on earth, we bribe a good friend of his, whom we know will not be refused . . . if you wish to gain favor with a more upright judge [God], grease the palms of the poor, which is the most important suit you can plead for your salvation.”⁴¹

The income, both official and extraofficial, of the magistrates of the *Audiencia*—ranging from £1,000 to £3,000 per year—placed them well within the limits needed to sustain a noble style of life during the seventeenth century.⁴² Naturally, judicial revenues were hardly limited to fees (and bribes) from the bench, as extraordinary grants and other political and social favors often supplemented strictly professional income.⁴³ Judges could also participate in a variety of business ventures. Probate and other property records reveal a wide range of investments by members of the legal elite, especially in real estate, seigneurial jurisdictions, and money lending. In

³⁸ Pujades, *Dietari*, ed. Josep M. Casas Homs, I (Barcelona, 1975), 165–66, 186, 245, 264, 325, 360–61, 393; ACA, Consejo de Aragón, *legajo* 225; Parets, “De los muchos sucesos dignos de memoria que han ocurrido en Barcelona y otros lugares de Cataluña,” ed. Cosme Parpal i Marqués, in *Memorial histórico español*, 20 (Madrid, 1888), 100, 150.

³⁹ *Constitutions y altres drets de Cathalunya*, 113–17. For *visitas* in general, see Elliott, *Revolt of the Catalans*, 89.

⁴⁰ Josep Mora i Pons, *Libro de memorias*, AHMB, Patrimonial, II-25, f. 120r.

⁴¹ Gaspar Sala, *Govern polítich de la ciutat de Barcelona* (Barcelona, 1636), f. 25v.

⁴² Elliott conjectured that £600 was a reasonable income for a gentleman in early modern Barcelona, while members of the higher nobility may have required as much as £2,000; see his “A Provincial Aristocracy: The Catalan Ruling Class in the Sixteenth and Seventeenth Centuries,” in *Homenaje a Jaime Vicens Vives*, 2 (Barcelona, 1967), 128–29. The annual earnings of the *Audiencia* judges placed them much closer to the upper aristocracy than to the gentry proper.

⁴³ For numerous *mercedes* or grants awarded to *Audiencia* magistrates, including patents of nobility, jobs for sons and other relatives, and gifts of baronies and other seigneurial jurisdictions, see ACA, Consejo de Aragón, *legajos* 483–88.

short, the magistrates of the superior courts of Catalonia—and by implication, the better-off members of the city's bar in general—could hope to garner substantial earnings from legal practice and judicial service.

Changes in the economic climate of the sixteenth and seventeenth centuries also influenced the size and economic strength of the legal elite. Recent studies have linked the rise in numbers, wealth, and social status of liberal professionals throughout early modern Europe to the economic expansion of the sixteenth century. According to this argument, an increase in agricultural production, commerce, and industrial output facilitated a dramatic growth in litigation that provided new opportunities for the legal profession. Conversely, economic depression in the seventeenth century led to a decline in litigation and a corresponding decrease in the number of lawyers.⁴⁴ The case of Barcelona, however, suggests restraint in drawing too close a connection between economic trends and the fortunes of the liberal professions.⁴⁵ Although the numbers and wealth of local lawyers did expand during the prosperous sixteenth century, that expansion continued during the economic contraction of the seventeenth. Furthermore, the recovery of the mid-eighteenth century was accompanied by a prolonged decline in both the relative size and political power of barristers as a group. The waning importance of the honored citizen oligarchy, replacement of the city council with new institutions administered by the central state, harsh repression of the principality's constitutional traditions and institutions—all of these developments eroded the prominence of the local legal elite.

To point this out is not to deny the influence of economic trends on the development of the professions. On the contrary, it suggests that a negative economic climate, ranging from stagnation to recession, may favor litigiousness. The shrinking of alternative sources of income may also have encouraged members of the elite to resort in increasing numbers to the practice of law.⁴⁶ It was surely no accident that the years of greatest economic downswing during the seventeenth century, 1635–75, coincided with a record number of matriculants, both noble and commoner, to the local bar.⁴⁷ The principal consequences of the demographic reverses and depressed economic conditions of the mid-seventeenth century were a further decline in commercial and industrial activity and a corresponding strength-

⁴⁴ Stone, "Educational Revolution," 68–71, "Social Mobility in England, 1500–1700," *Past and Present*, 33 (1966): 40–43; Peter Clark, *English Provincial Society from the Reformation to the Revolution* (Sussex, 1977), chap. 9; Prest, *Inns of Court*, 22–25; and Kagan, *Lawsuits and Litigants*, esp. pt. 1. None of these scholars argued that economic developments were the sole influence on the fortunes of the legal profession. They have also singled out the growth of state bureaucracies and the decline (at least in England) of church litigation as contributing factors.

⁴⁵ Barcelona barristers rarely dealt directly with strictly commercial disputes, which local merchants themselves adjudicated in the consulate. The surviving *Audiencia* suits from the seventeenth century in the ACA deal almost exclusively with jurisdiction and property settlements, such as transfers of land rights, contested inheritances and wardships, and other matters touching the seigneurial and rentier economy of the upper classes.

⁴⁶ Paul Lucas argued that in eighteenth-century England admissions to law schools of students from lower classes rose during years of economic depression, because of declining opportunities in other sectors of the economy; see his "A Collective Biography of Students and Barristers of Lincoln's Inn, 1680–1804: A Study in the Aristocratic Resurgence of the Eighteenth Century," *Journal of Modern History*, 46 (1974): 227–62. In the case of early modern Barcelona, one can apply this argument to the urban gentry as well.

⁴⁷ *Matrícula dels jurisperits*, AHMB, Vegueria, ser. 15.

ening of the rentier and professional sectors of the economy. The latter formed the keystone of a system designed to produce small but regular returns on capital investment—a system that, in short, placed overriding emphasis on security of income. The municipal elite's patterns of investment, closely resembling the so-called betrayal of the bourgeoisie—an anachronistic term reflecting the entrepreneurial orientation and values of the nineteenth and twentieth centuries—constituted a rational, if minimal, response to an economic climate that had shifted from moderate growth to depression.⁴⁸ The study and practice of law played a crucial role in the urban elite's gradual retreat from commercial and other high-risk investments toward more secure, if less “dynamic,” sources of income.

THE RISING DEMAND FOR LEGAL SERVICES and the relative profitability of bar and bench helped swell the numbers and wealth of local jurists. Yet the expanding income of the upper ranks of the legal profession did not constitute its sole attraction. On the contrary, many were attracted to the exercise of law because it promised both upward social mobility and increased access to political power.

That “letters” frequently accompanied arms along the slippery path of social ascent was a commonplace among early modern writers. The seventeenth-century Catalan jurist Andreu Bosch wondered at the “many routes” by which contemporaries “rise up slowly from the lower estates to the ranks of burgesses, honored citizens, and those of letters, the ones most frequented and honored in the larger cities, although military titles are more highly regarded.”⁴⁹ All the same, little is known of the social origins of the barristers and judges of early modern Barcelona. Rapid ascents up the social ladder were all the more notable because abnormal. One example was that of the Franquesas, the prototype of Gil Blas's “apocryphal nobility,” who because of their lowly origins were readily scorned after they fell from power. Only a handful of jurists at most rose *within* the ranks of the profession from attorney to barrister.⁵⁰ Doctors in law of lower-class origin came from the families of merchants and the wealthier guild masters. Hence, many honored citizens and *gaudints* opposed admitting aristocrats to the municipal government in 1621. Since many “merchants and guild masters have sons, brothers, or other close relatives who are jurists or physicians, and as such are included within the honored citizen estate,” it was argued, “entry of the nobles would diminish their chances of holding office.”⁵¹

⁴⁸ Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, trans. Siân Reynolds (New York, 1976), 725–36. Other historians have placed greater emphasis on the “rationality”—in this case, a balance between profit maximalization and security of investments—of the economic proclivities of similar early modern urban elites. In particular, see H. Soly, “The ‘Betrayal’ of the Sixteenth-Century Bourgeoisie: A Myth? Some Considerations of the Behavior Patterns of the Merchants of Antwerp in the Sixteenth Century,” *Acta Historiae Neerlandicae*, 8 (1975): 31–49.

⁴⁹ Bosch, *Summari index o epitome dels títols de honor de Cathalunya, Rosselló, y Cerdanya* (Perpignan, 1628), 370.

⁵⁰ I have located only nine men who rose from attorney to barrister during the seventeenth century.

⁵¹ *Memorial de lo estament dels ciutadans*, AHMB, Consell de Cent XVIII, *Deliberacions*, vol. 130 (1621), article 6 following f. 37v. The consolidation in the early modern Catalan countryside of the inheritance system based on the *hereu*, or single designated heir, stimulated the recruitment of younger sons, who were not heirs, into the liberal professions. Hence, the local barrister Francesch Soler emphasized the way in which these professions served to integrate recent migrants into the city; see his *Discurs ab lo qual entén provar que no convé nis pot en casa de la ciutat de Barcelona admetre los qui tenen nom de don* (Barcelona, 1621), 23.

Substantial advantages accrued to members of the liberal professions in early modern Catalonia. Among these was the personal nobility that university education conferred upon its recipients. By the sixteenth century, holders of advanced degrees enjoyed gentry status during their own lifetimes. As distinct from the experience of early modern Castile, this *gaudint* nobility was far from hypothetical. Throughout the seventeenth century, the aristocratic estate in parliament—the supreme arbiter of noble affairs in the principality—frequently defended the immunities and privileges of doctors in law.⁵² The considerable prestige of the Catalan legal tradition also enhanced the prominence of the local forensic profession. Law provided a crucial focus of intellectual endeavor in early modern Barcelona. Short-title catalogues reveal the overwhelming predominance of religion and jurisprudence in the output of local print shops.⁵³ Jurists such as Joan Pere Fontanella and Acaci Ripoll enjoyed a European-wide reputation for their works on civil law, bolstered by editions of their works issuing from presses in Lyon, Venice, and Basel.⁵⁴ When the city's leading royal official was asked in 1787 to list "the persons signal in virtue, letters, arms, and illustrious deeds" in Barcelona's history, the only writers named were twelve jurisconsults, ten of whom wrote during the seventeenth century.⁵⁵

Law degrees also provided access to positions of public authority. Admission of doctors in law and medicine to the municipal government of Barcelona was initially formalized in the royal privilege of 1455. Entry into the higher ranks of the legal profession also opened other paths to power through the considerable role its members played in local political movements. The extraordinary vitality of the Catalan constitutional tradition ensured that contemporary issues were raised and discussed within a predominantly legal framework. The frequent clashes between royal authority and local institutions almost always involved endless rounds of legal arguments. Few jurists of the period, especially those living during the troubled decades of the mid-seventeenth century, could avoid taking sides in the protracted defense of local constitutional rights against encroachments by royal officials. The growing disaffection of the ruling classes of the principality directly involved both the professional and personal interests of a large portion of the city's legal elite. In many respects, the Catalan uprising of 1640, like the parliamentary struggle in England and the French *Fronde* of the same decade, was a "revolt of the lawyers."⁵⁶

⁵² See, for example, the *Dietari del bras militar*, ACA, Generalitat, ser. G-68, vol. 4, July 24, 1683.

⁵³ Marià Aguiló, *Catálogo de obras en lengua catalana impresas desde 1474 hasta 1860* (Madrid, 1923); and Jaume Andreu, *Catálogo de una colección de impresos referentes a Cataluña*, s. XVI-XIX (Barcelona, 1902).

⁵⁴ See, for example, Jaume Cancer, *Variarum Resolutionum Iuris Principatus Cathaloniae* (Lyon, 1683; Antwerp, 1689); Acaci Ripoll, *Variae Iuris Resolutiones* (Lyon, 1630), *Practicabilia Commentaria et Quotidianae Praxi* (Cologne, 1617); Fontanella, *Sacri Regii Senatus Cathaloniae Decisiones* (Venice, 1640; Lyon, 1668; Geneva, 1662 and 1735); and *Tractatus de Pactis Nuptialibus* (Geneva, 1659 and 1752–79; Lyon, 1667 and 1719). The most famous Catalan contribution to the European legal system was the late medieval maritime law code of the *Consolat del Mar* (Merchant Consulate), which went through numerous printings during the early modern period. See Robert S. Smith, "The *Llibre del Consolat del Mar*: A Bibliography," *Law Library Journal*, 33 (1940): 387–96; and John P. Reeder, "Bibliografía de traducciones al castellano y catalán, durante el s. XVIII, de obras de pensamiento económico," *Moneda y crédito*, 126 (1973): 60, 70–71.

⁵⁵ Francisco de Zamora, *Diario de los viajes hechos en Cataluña*, ed. Ramon Boixareu (Barcelona, 1973), 474–77.

⁵⁶ For the political activities of lawyers during this period, see Christopher Hill, *The Intellectual Origins of the English Revolution* (Oxford, 1965), chap. 5; Brian P. Levack, *The Civil Lawyers in England, 1603–41: A Political Study* (Oxford, 1973); and Stephen D. White, *Sir Edward Coke and the Grievances of the Commonwealth, 1621–28* (Chapel Hill, 1979).

The canonist Pau Claris and the famed juriconsult Joan Pere Fontanella were but two of many spokesmen for the revolutionary cause drawn from the ranks of the legal profession.⁵⁷ The leading role played by barristers within the revolt is not surprising. Throughout Europe jurists led the struggle for the protection of local and particularist privileges against centralizing monarchies.

The reverse side of this coin was the firm, if not always enthusiastic, support for royal policy found among an important sector of Barcelona's legal profession—the judges of the *Audiencia*. These magistrates wielded a great influence on the local scene.⁵⁸ Especially crucial was their role as a permanent body of political advisers providing continuity within the royal administration between frequent changes in viceroys. The peculiar political climate of the seventeenth century also augmented the responsibilities of the *Audiencia*. The institutional vacuum created by the failure to convoke or conclude successfully any parliamentary session from 1599 to 1702 substantially increased the tribunal's decision making. This "blockage" of traditional channels of legislation enhanced the role played by judicial interpretation as a substitute for positive law.⁵⁹ It was surely no coincidence that the seventeenth century witnessed the publication of numerous compilations of *Audiencia* decisions, which by the end of the century were being updated nearly every decade. In short, the increased political responsibilities of *Audiencia* judges during the early modern period went far beyond chasing bandits and drumming up soldiers and supplies in the countryside. The paralysis beginning in 1599 of the traditional constitutional apparatus opened up new possibilities for magistrates to influence local politics through judicial interpretation.

The prominent role played by the *Audiencia* in Catalan public life was reflected in a variety of ways. The precedence accorded judges over noblemen in Barcelona's Corpus Christi procession was one symbol of their relative importance.⁶⁰ At times magistrates paid a heavy price for their visible role in public affairs. As related earlier, five of fifteen *Audiencia* judges were lynched in the Corpus Christi and Christmas Eve riots of 1640.⁶¹ Yet care should be taken not to let distinctions of this sort distort appraisals of the overall significance of the bench. At no point did the *Audiencia* of early modern Catalonia enjoy the same powers and prerogatives as the most activist tribunals of seventeenth-century Europe, that is, the parlements of France. The greater dependence of the Catalan judges on the royal will—largely because of the absence of the French *paulette*, or venal system of hereditary offices—meant that, despite the *Audiencia*'s attempt to "moderate" constitutional conflict during the 1630s, it never joined local opposition to crown policy as did many such

⁵⁷ Elliott, *Revolt of the Catalans*, 363, 470–78.

⁵⁸ For the political activism of the *Audiencia* magistrates, see Elliott, *Revolt of the Catalans*, 86–90, 367–68.

⁵⁹ Lluís de València, *Ilustración a la Constitución VII*, esp. chaps. 2, 4. *Doctrines de doctors* were fixed as a source of Catalan law in 1599, whereas they had been eliminated from Castilian jurisprudence in 1504; see *Constitutions y altres drets de Cathalunya*, 89; and Francisco Tomás y Valiente, *Manual de historia del derecho español* (Madrid, 1979), 320. Although "doctrines" in this case referred specifically to both the interpretations compiled in the *Corpus Iuris Civilis* and the later medieval glosses, contemporary jurisprudence was not excluded from consideration.

⁶⁰ Fontanella listed the ceremonial distinctions accorded to *doctores Regii Concilii*; *Tractatus de Pactis Nuptialibus*, 1 (Lyon, 1617), 7.

⁶¹ Elliott, *Revolt of the Catalans*, chap. 15. Of special interest is the account of the Barcelona revolt of June 7, 1640—the famous "Bloody Corpus"—by Ramon Rubí i Marimon, an *Audiencia* judge; see his *Relación*, ff. 74r–157v.

tribunals in France.⁶² Close scrutiny of prior legal and political activity at the moment of nomination to office, combined with the substantial rewards to be garnered by ongoing support of royal policies, frustrated the emergence of the *Audiencia* judges as a coherent body independent of the monarch.⁶³

The unique political configuration of sixteenth- and seventeenth-century Catalonia lent important stimulus to the development of the legal profession in its capital city. The special conditions governing entry into the municipal administration of Barcelona sustained the remarkable number and public prominence of its jurists, especially in comparison with the rest of Spain. While the economic opportunities and social status of lawyers increased throughout Europe beginning in the later Middle Ages, Barcelona provided a unique opportunity for them to pursue public service. The city's political structures, and the response of its ruling classes to a stagnant economic climate, were the most important specific causes of the growing influence of the legal profession. All the same, the more anomalous features of early modern Barcelona should not be exaggerated, nor should its special role within the civic life of Habsburg Spain obscure the fact that, in many respects, developments in local society and politics resembled changes found elsewhere in Europe. These included a fundamental transformation of the traditional urban oligarchy. This redefinition combined the displacement from the country to the city of the earlier, feudal aristocracy with the selective absorption of liberal professionals into the ranks of the civic patriciate.⁶⁴ The new, cohesive ruling class emerging from this amalgam could thrive solely in an atmosphere marked by a shared aristocratic culture—one that separated members of the elite from their social “inferiors.”⁶⁵ Cities like Barcelona provided such a venue. Viewed from this general perspective, the experience of its lawyers was lodged firmly within the mainstream of early modern society. Despite specific variants in their sources, the power and wealth of Barcelona's jurists would have occasioned little surprise—albeit much envy—among their counterparts in the rest of Western Europe.

THERE WAS NOTHING INEXORABLE OR CONTINUOUS in the “rise” of this legal elite. Eighteenth-century Barcelona witnessed a significant contraction in the relative size and public role of its barristers. The atrophy or outright abolition of the social ranks and political institutions nurturing the previous expansion of the legal profession

⁶² On the political comportment of Catalan magistrates during the 1630s, see Elliott, *Revolt of the Catalans*, chaps. 10–16. On the politics of the French parlements during this period, see Jonathan Dewald, “Magistracy and Political Opposition at Rouen: A Social Context,” *Sixteenth-Century Journal*, 5 (1974): 66–78; A. Lloyd Moote, *The Revolt of the Judges: The Parlement of Paris and the Fronde, 1643–52* (Princeton, 1971); Albert N. Hamscher, *The Parlement of Paris after the Fronde, 1653–73* (Pittsburgh, 1976); and Sharon Kettering, *Judicial Politics and Urban Revolt in Seventeenth-Century France: The Parlement of Aix, 1629–59* (Princeton, 1978).

⁶³ There are some interesting parallels between the behavior of Catalan and English judges during the political crisis of the 1630s. On the latter, see, for example, Margaret A. Judson, *The Crisis of the Constitution: An Essay in Constitutional and Political Thought in England, 1603–45* (New Brunswick, 1949), 107–70.

⁶⁴ Carlo M. Cipolla, “The Professions: The Long View,” *Journal of European Economic History*, 2 (1973): 37–53. In many Mediterranean cities, the status of lawyers was already quite high by the later Middle Ages. See, for example, Martines, *Lawyers and Statecraft*, chaps. 2, 3; and André Gouron, “Le rôle social des juristes dans les villes méridionales au Moyen Âge,” in *Villes de l'Europe méditerranéenne et de l'Europe occidentale* (Nice, 1969), 55–67. In some cities like Lucca and Nuremberg, however, lawyers were expressly excluded from local government. See Marino Berengo, *Nobili e mercanti nella Lucca del cinquecento* (Turin, 1965), 53–64; and Gerald Strauss, *Nuremberg in the Sixteenth Century* (Bloomington, 1976), 58.

⁶⁵ See James S. Amelang, “Public Ceremonies and Private Fêtes: Social Segregation and Aristocratic Culture

contributed to this decline. Other social sectors, ranging from the Castilian political and military bureaucracy to flourishing native merchant and manufacturing groups, assumed the role of protagonists in urban affairs. Ironically, as a consequence of the shifts in the structures of local power and authority, the close of the eighteenth century saw a marked increase in the “professional” consciousness of Barcelona’s lawyers. This change in attitude—similar to contemporary French jurists’ views of themselves as “self-made men” in contrast to the wastrel *oisifs* of the rentier nobility—found institutional expression in the establishment in 1777 of an Academy of Jurisprudence, the forerunner of the present-day Barristers’ Association.⁶⁶ Its early spokesmen placed renewed emphasis on the contrast between the professional knowledge of the barrister and the ignorance of his client, coupled with an insistence on law as a studiously theoretical form of knowledge divorced from the charges of gouging and fraud unsparingly levelled by contemporaries.⁶⁷ These views formed part of the unrelentingly “professional” mentality that achieved its greatest flowering in middle-class ideologies of the nineteenth and twentieth centuries.⁶⁸ The new-found cohesion of the legal community ensured that it would not suffer a full eclipse as the profession waned. The foundations that underlay the continued social and political prominence of the legal profession, built in the early modern period, proved to be one of its enduring legacies.

in Early Modern Barcelona,” in Gary W. McDonogh, ed., *Conflict in Catalonia: Images of an Urban Society* (forthcoming).

⁶⁶ François Fossa, *Mémoire sur la prétendue noblesse des bourgeois majeurs de Perpignan et de Barcelone* (Toulouse, 1777), 2; and Berlanstein, *Barristers of Toulouse*, 28–31. For the founding of the barristers’ guild, see Antoni M. Borrell i Soler, “La Academia de Legislación y Jurisprudencia,” *Revista Jurídica de Cataluña*, 72 (1955): 394–407.

⁶⁷ Vicente Domenech y Sabater, *Discurso sobre las obligaciones del abogado* (Barcelona, 1779); Manuel Barba y Roca, *Discurso sobre los pleitos* (Barcelona, 1781); Francisco Sans de Monrodón y de Sala, *Oración inaugural sobre las excelencias del juez* (Barcelona, 1781); and Miguel Juan de Magarola, *El Abogado perfecto* (Barcelona, 1789).

⁶⁸ Angel Ossorio y Gallardo’s *El Alma de la toga* (Madrid, 1922) is a modern classic of this genre.

Research Note

Great Britain's Secret Convict Trade to America, 1783–1784

A. ROGER EKIRCH

DURING THE FINAL MONTHS OF THE AMERICAN REVOLUTION, the British government decided to reopen the convict trade to its former colonies. Transportation, for many years Britain's favorite punishment for serious crime, had ended when vessels containing felons were refused entry into American ports shortly after the outbreak of fighting at Lexington and Concord. In mid-1783, however, the coalition ministry of Lord Frederick North and Charles Fox, alarmed by a crime wave in London and much of the countryside, resolved to revive transportation, although American opposition to the trade was longstanding and the Treaty of Paris as yet unratified. In response to a proposal from North, George III wrote on July 12, "Undoubtedly the Americans cannot expect nor ever will receive any favour from Me, but the permitting them to obtain Men unworthy to remain in this Island I shall certainly consent to." By then, North had enlisted George Moore, a London merchant, whose vessel, the *George*, was ready within weeks to sail with a cargo of 143 prisoners. Moore was promised five hundred pounds from the English Treasury and whatever profits the convicts fetched as servants.¹

Historians have known that Moore's cargo was eventually unloaded in Maryland but not the fascinating details that attended this episode. The convicts had to be imported by means of an elaborate hoax and with the secret assistance of one of Maryland's prominent merchants, George Salmon. In the Manuscripts Division of the Library of Congress is a letterbook kept by Salmon, which contains a remarkable series of letters written to Moore over a period of twelve months, outlining plans for bringing convicts into Maryland. The correspondence is exceptionally frank in describing their efforts to deceive state authorities through a variety of subterfuges. Salmon's letters also reveal that their scheme was a colossal failure, which discouraged the revival of transportation to America and forced Britain to find a new location for much of its criminal population.

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¹ George III to North, July 12, 1783; North to George III, [July 11, 1783], reprinted in Sir John Fortescue, ed., *The Correspondence of King George the Third*, 6 (London, 1928): 415–16; and North to Lords of Treasury, November 5, 1783, Public Record Office, London [hereafter, PRO], State Papers 44/330/313 [hereafter, SP].

BEFORE THE REVOLUTION DREW TO A CLOSE, the English faced an upsurge in domestic violence. On top of the bloody Gordon Riots of 1780, a crime epidemic erupted, more menacing to England's welfare than anything the American navy had accomplished during eight years of war. An economic depression, widespread unemployment, and the demobilization of thousands of hardened veterans were the cause. "Almost all Peoples minds are more occupied about the Depredatory Cruelties and Robberies that are continually carrying on," claimed a London resident in September 1782, "than they are about . . . the Event of Our Arms."² Since passage of the Transportation Act in 1718, more than fifty thousand convicts had been banished to the colonies, but because the War for Independence abruptly ended the flow the criminal justice system was left in chaos. Beginning in 1776, hundreds of malefactors were housed aboard the "hulks," weather-beaten warships moored in the Thames, from which prisoners constantly tried to escape and where the mortality rate approached one in four during the first three years.³ Other felons were confined to prisons and county gaols, though these, too, became overcrowded; Edmund Burke described them as "nests of pestilence."⁴ An attempt to settle convicts in West Africa failed. Another solution was an unprecedented increase in the use of the death penalty; George III and his ministers became less willing to extend executive clemency to condemned prisoners.⁵ As prisons overflowed with diseased and dangerous criminals, breakouts became common. "The Public will be happy to get rid of them at any Rate," affirmed a London magistrate in 1782. "The grand consideration," the British governor of Cape Coast Castle in Africa echoed, "seems to be, *to get them out of Europe at all Events.*"⁶

In the decades preceding the Revolution, Britain's policy of transporting criminals provoked heated protests by colonial governments. Several colonies attempted to impose prohibitive taxes on incoming felons. Typical of prevailing sentiment was the bitter comment in 1731 by the provincial council of Jamaica, "[If] it be prudence in England to banish rogues; it must certainly be prudence here to endeavour to keep them out." Benjamin Franklin, who in 1751 advocated exporting rattlesnakes to Mother England, described transportation as "insult and

² John Bindley to Thomas Townshend, September 16, 1782, PRO, Home Office Papers 42/1/138 [hereafter, HO]; Douglas Hay, "War, Dearth, and Theft in the Eighteenth Century: The Record of the English Courts," *Past and Present*, 95 (1982): 142–45; J. M. Beattie, "The Pattern of Crime in England, 1660–1800," *Past and Present*, 62 (1974): 93–95; and Michael Ignatieff, *A Just Measure of Pain: The Penitentiary in the Industrial Revolution, 1750–1850* (New York, 1978), 82.

³ Ignatieff, *Just Measure of Pain*, 80–81; Duncan Campbell to Earl of Suffolk, October 1, 1778, Mitchell Library, Sydney, Australia, Duncan Campbell Business Letter-Book, vol. 2: 248–50. For accounts of transportation during the eighteenth century, see Abbot Emerson Smith, *Colonists in Bondage: White Servitude and Convict Labor in America, 1607–1776* (Chapel Hill, 1947), 110–35; Richard B. Morris, *Government and Labor in Early America* (New York, 1946), 323–37; Kenneth Morgan, "The Organization of the Convict Trade to Maryland, 1718–1776," *William and Mary Quarterly*, 3rd ser. (forthcoming); and A. Roger Ekirch, "Bound for America: A Profile of British Convicts Transported to the Colonies, 1718–1775," *William and Mary Quarterly*, 3rd ser. (forthcoming).

⁴ Ignatieff, *Just Measure of Pain*, 84–85; and Burke, as quoted in *The Parliamentary History of England*, 25 (London, 1814): 431.

⁵ Mollie Gillen, "The Botany Bay Decision, 1786: Convicts, not Empire," *English Historical Review*, 97 (1982): 745–46; Ignatieff, *Just Measure of Pain*, 86–87; and Douglass Hay, "Crime, Authority, and the Criminal Law: Staffordshire, 1750–1800" (Ph.D. dissertation, University of Warwick, 1975), 520–21.

⁶ Sampson Wright to [?], August 24, 1782, HO 42/1/290; Richard Miles to [?], February 1, 1783, PRO, Treasury Papers 70/33/53 [hereafter, TJ]; and Ignatieff, *Just Measure of Pain*, 82–85.

contempt, the cruellest perhaps ever one people offered another.”⁷ But alternatives were few, and by early 1783 crown officials, particularly the Treasury, were resigned to a resumption of the trade. Some courts were already ordering criminals transported to “his Majestys Colonies and Plantations in America.”⁸ But none of the sentences could be carried out until the king gave North his approval in early summer and the contract was concluded with George Moore. Taken from London's Newgate prison and the hulks, most of the transported were sentenced to seven years exile for having committed grand larceny and other noncapital felonies.⁹ Their destination was Maryland, to which large numbers of convicts had been sent before the Revolution and sold as servants.

No one could later fault Moore's diligence. Before the *George* got underway, he made careful preparations for its arrival in Baltimore. Ready to assist in the marketing of the vessel's cargo was George Salmon, a leading Whig merchant with an eye for the main chance. In April 1783, with the war nearly over, he had written Moore about renewed opportunities for selling British felons to local planters. The two men had never met, but Salmon had been urged to write by Moore's brother, Phillip, a merchant in Philadelphia. “I don't know any thing would bring more money here,” Salmon assured George Moore, “than a parcel Servants or Convicts which was formerly a good business.” Nor was Salmon unduly concerned that convicts might not be permitted to land in Maryland, although he certainly was aware of earlier opposition to their importation and of the growth in anti-British sentiment during the Revolution. No law yet existed to prohibit the trade in felons, and Salmon had confidence in his political connections. At one point he assured Moore, “The Governor's Council are Gentlemen whom I am well acquainted with, so that I have both Law and interest to back me.” “I would be far,” he added, “from leading you into a Scrape.”¹⁰ Still, caution dictated that the mission of the *George* be disguised. Salmon and Moore agreed to market the ship's cargo as indentured

⁷ Council of Jamaica to Governor Robert Hunter, enclosed in Hunter to Duke of Newcastle, December 15, 1731, reprinted in Cecil Headlam, ed., *Calendar of State Papers, Colonial Series*, 38 (reprint edn., Nendeln, Liechtenstein, 1964): 377; *London Chronicle*, May 12, 1759, reprinted in Leonard W. Labaree, ed., *The Papers of Benjamin Franklin*, 8 (New Haven, 1965): 351; “Americanus,” *Pennsylvania Gazette*, Philadelphia, May 9, 1751; and Smith, *Colonists in Bondage*, 119–22.

⁸ Maryland Hall of Records, Annapolis, Baltimore County Convict Record, 1770–[1783], 388–89 [hereafter, BCCR]; Wilfrid Oldham, “The Administration of the System of Transportation of British Convicts, 1763–1793” (Ph.D. dissertation, University of London, 1933), 225; and J. M. Beattie, *Crime and the Courts in England, 1660–1800* (forthcoming). I am grateful to John Beattie for allowing me to see advance copies of several chapters from his manuscript. For an early plan submitted by Duncan Campbell, the superintendent of the hulks, for transporting convicts to North America, which includes an undated estimate of expenditures, see Proposal by Duncan Campbell, December 27, 1782, T 1/581/135–37.

⁹ North to Lords of Treasury, November 5, 1783, SP 44/330/313; Oldham, “Transportation of British Convicts,” 228; and BCCR, 383–89. North and George III were aware that convicts might not be welcome in America; they agreed that Moore would “land them in Nova Scotia,” if they could not be “landed in any part of the United States”; North to George III, [July 18, 1783], reprinted in Fortescue, *Correspondence of George III*, 418.

¹⁰ Salmon to George Moore, April 30, October 3, 1783, Library of Congress, Washington, Peter Force Collection, ser. 8D, Woolsey and Salmon letterbook [hereafter, WSLB], 478, 494; and Joshua Johnson to John Jay, August 22, 1783, reprinted in Richard B. Morris, ed., *John Jay: The Winning of the Peace*, 2 (New York, 1980): 572–73. Unfortunately, four letters from Salmon to George Moore, dated June 11, June 29, July 20, and August 17, 1783, were not recorded but only noted in the letterbook. See WSLB, 485–86. The Maryland merchant James Cheston, a onetime trader in convicts, described Salmon as a “Very high Whig”; Cheston to William Randolph, February 18, 1785, Maryland Historical Society, Baltimore, Cheston-Galloway Papers [hereafter, CGP].

servants, to rename the vessel, and to announce in London that Nova Scotia was its destination. Once at sea, the shipmaster, Thomas Pamp, would sail for Baltimore. Who would refuse refuge to a wayward vessel in distress, should the true identity of its passengers be discovered?¹¹

Disaster struck early. Hardly had the ship, newly rechristened the *Swift*, cleared the Thames in late August before the convicts rebelled and ran it aground off the Sussex coast. More than one-quarter of them escaped, and authorities spent anxious days rounding up as many fugitives as possible. A few were later executed.¹² The vessel was forced to put into Portsmouth, where it stayed for a month. But Moore was undaunted, and, once the delay was over, Pamp pressed on with just over one hundred convicts still aboard. Salmon also remained enthusiastic. After learning of the mishap, he urged Moore to dispatch a second vessel with convicts in the spring. Affirming that “money is to be made by this business,” he stressed the importance of following their original scheme. “Secrecy and good conduct will be highly necessary. . . . They must all be cleared out with you for Halifax. Should you mention this place [Maryland] the Alarm would be taken here and a Law soon passed to prevent their being imported—at present no such Law is in being and if we only act with proper caution the business might be carried on for a great length of time without any person suspecting—Servants is the word and they may be imported every day in the Year.” In a subsequent letter, Salmon added, “It would always be necessary to change the Vessels name after she had taken them on board, as your News papers comes over here and every matter put into ours. . . . Although Congress have made this state the place of their residence and that they are within 30 miles of this Town, yet don’t be alarmed on that account. They can’t enact a single Law and as to our House Assembly I am sure they never will have reason to pass one without we act in too glaring a manner.”¹³

On Christmas eve the *Swift* finally arrived in Baltimore harbor—only to encounter a new problem. Although Pamp duly informed authorities that a shortage of provisions had prevented sailing to Nova Scotia,¹⁴ Maryland lawmakers had received word from abroad detailing the entire hoax. The informant was Matthew Ridley, an American merchant in Paris with close friends and business associates in Maryland, who alerted Governor William Paca to the scheme in mid-September. On December 31, Salmon wrote to Moore, “You Can’t Imagine The Anxiety I have Suffered since the arrival of this Ship for fear a Stop should Be Put to the Sale. Would you Believe that a letter was received by our Governor and

¹¹ Joshua Johnson to John Jay, August 22, 1783, reprinted in Morris, *John Jay*, 572–73; Matthew Ridley to William Paca, September 12, 1783, Massachusetts Historical Society, Boston, Matthew Ridley Collection, case 8; and James Cheston to William Randolph, February 23, 1784, February 18, 1785, CGP. Ironically, the following London report, dated August 9, 1783, and giving the vessel’s new name, appeared in the *Maryland Gazette*, Annapolis, on November 13: “On Saturday morning early about ninety convicts, under sentence of transportation in Newgate, were put on board a lighter at Black Friars, which proceeded with and put them on board the *Swift*, Captain Pump, lying at Blackwall, for their reception, and bound to Nova Scotia (the land of frost and freedom).”

¹² Andrew Knapp and William Baldwin, eds., *The Newgate Calendar, comprising interesting memoirs of the most notorious characters who have been convicted of outrages on the laws of England, etc.*, 3 (London, 1828): 124–25.

¹³ Salmon to Moore, November 22, December 20, 1783, WSLB, 503–04, 509. In late 1783 Congress was meeting in Annapolis.

¹⁴ James Cheston to William Randolph, February 23, 1784, CGP.

Council some time Ago. . . , giving an Exact discription of the Ship the Masters Name and who she was Consigned to in Baltimore." The state assembly in Annapolis had been in session, and its members were aroused "into a Flame on the Supposition that England should Empty Their Jails on them." Yet no ban was enacted. On learning from Ridley of the prisoner uprising at sea, legislators assumed that the danger had passed. By the time they heard on Christmas day of the *Swift's* arrival in Baltimore, they were adjourning to return to their plantations. "They did not Stay to Pass a Law," reported Salmon, "So that I am now Selling them Publickly without Dread or fear." Within a few days, he succeeded in selling as many as twenty convicts for prices ranging from twenty to thirty-five pounds. Although their real identities were known—they were even registered as transported felons by a local court—all were advertised as servants indentured for five years.¹⁵

Buoyed by his good fortune, Salmon again urged that Moore ready a cargo for the spring, though he instructed that it arrive before the assembly was due to reconvene in mid-April. "Should you send me a parcel before then," he wrote, "there is no doubt but I can readily dispose of them." He suggested to Moore an even more elaborate subterfuge for the next shipment. The prisoners should be routed through Ireland, so that they might be disguised as Irish servants. "I still think they might be imported notwithstanding any Law that may be in force, but then secrecy and good conduct must be closely attended to, to clear them out with you for Halifax. No goods or Letters to be taken on board for this Town, the Vessel to touch at Cork, there take a few bbls. provisions, change her name and clear out from that, the Captain to indent them on the Passage for 4 years and then I would sell them as Irish servants and the indentures would be as good as any were this scheme put in execution."¹⁶

Within weeks, however, prospects for smuggling convicts into Maryland changed dramatically. For one thing, sales aboard the *Swift* suddenly slowed. Local demand for labor was low in the winter months, and unusually heavy snows and frigid temperatures kept what buyers there were confined to their homes. The *Swift* became trapped by ice "thick enough," Salmon described, "to bear a hogshead [of] Tobacco." Of 104 convicts only 30 were sold by mid-January, and a scarcity of hard currency forced Salmon to extend credit. Two weeks later, 72 convicts still were on board. Some were gravely ill, and others were on the verge of escaping across the ice. With costs mounting for food, clothing, and medical care, a clearly disgusted Salmon wrote Moore on February 4, "I [have] thought several times it would be almost as good to let the Villains go on shore and so have done without them . . . if I find I cannot sell them for some price or other, I will turn them adrift."¹⁷

¹⁵ Salmon to Moore, December 31, 1783, WSLB, 510–11; Paca to General Assembly, December 2, 1784, [1783], reprinted in William Hand Browne et al., eds., *Archives of Maryland*, volume 48: *Journal and Correspondence of the State Council of Maryland, 1781–1784*, ed. J. Hall Pleasants (Baltimore, 1931), 484; Ridley to Paca, September 12, 1783, BCCR, 383–89; and *Maryland Gazette: or, The Baltimore General Advertiser*, January 2, 9, 16, 1784. Ridley evidently learned of the expedition from John Jay, then in Paris for negotiation of the peace treaty, who in turn was informed by Joshua Johnson of London. See Johnson to Jay, August 22, 1783, in Morris, *John Jay*, 572–73.

¹⁶ Salmon to Moore, December 31, 1783, WSLB, 510–11.

¹⁷ Salmon to Moore, January 15, February 4, March 15, 1784, WSLB, 512–14, 517–22. On runaway convicts, see *Maryland Gazette: or, The Baltimore General Advertiser*, January 16, March 5, 1784.

In the meantime, area residents had grown increasingly alarmed, partly because some convicts purchased locally had already run away from their masters. Angry state politicians, despite Salmon's best efforts to distribute "Porter and Cheese" among his "friends" with "influence in the House [of] Assembly," vowed to ban incoming felons as soon as the legislature reconvened. To make matters worse, several lawyers declared that convicts could not be forced to become servants, and the luckless merchant was even hauled with Pamp before a grand jury for questioning. "I have been plagued beyond expression," he informed Moore in March.¹⁸

By spring, all but six or eight of the felons had finally been sold, and the *Swift* was ready to take on a new cargo for its return to England. Both merchants, however, suffered serious losses. Besides having to shoulder the heavy expense of boarding so many prisoners, they received little from local purchasers. Once convicts began running away, buyers stubbornly refused to honor their debts. The only bright side of the transaction was the English Treasury, which, apparently untroubled by the whereabouts of the convicts as long as they were beyond the Atlantic, paid Moore the promised five hundred pounds.¹⁹

NO DOUBT THE DISASTROUS VOYAGE of the *Swift* seriously dampened the enthusiasm of British officials for resuming convict transportation to America, but it was not the final act in the Moore-Salmon farce. After the fiasco with the first cargo, Salmon lost his enthusiasm for another shipment of felons. In mid-January, he questioned the feasibility of sending them by way of Ireland and in early February flatly told Moore to "send no more." The assembly, he feared, would soon enact a ban, and there were "so many busy people that it would be impossible to sell a Cargo" of convicts routed through Ireland "without having the whole plan discovered."²⁰ By then, however, Moore was in the final preparations for a second voyage. At what point he received Salmon's discouraging advice remains a mystery, but in early April, with backing from Whitehall, his vessel, the *Mercury*, sailed with 179 felons. Very little is known about the ship's voyage or even its exact destination, except that again prisoners rebelled, and, when the vessel reached the American coast after a long and harrowing passage, no port would permit its entry. Finally, the convicts were unloaded in British Honduras, much to the anger of local settlers. "We are the

¹⁸ Salmon to Moore, January 15, February 4, March 15, 1784, WSLB, 512–14, 517–22.

¹⁹ Salmon to Moore, February 4, March 15, 1784, *ibid.*, 517–22; Moore to Treasury, July 13, 1786, HO 42/9/565; and North to Treasury, November 5, 1783, SP 44/330/313. Whether Salmon ever received a portion of the £500 is not known. He suffered an additional loss of over £200 from goods aboard the *Swift* that were intended for sale along with the convicts. Besides being damaged, the goods were not readily marketable because of the ship's late arrival. Salmon to Moore, January 15, February 4, 1784, WSLB, 512–14, 517–19.

²⁰ Salmon to Moore, January 15, February 4, 1784, WSLB, 512–14, 517–19. Whether Maryland finally passed a convict ban is doubtful. The legislature did not meet in April, as Salmon anticipated, and no record exists of the enactment of such a law during the immediate postwar years. In late 1784, when the assembly again met, members probably concluded either that the danger of convict importations had passed or that legislation was not needed to prevent the arrival of future vessels. In early 1785 James Cheston advised a former partner in the convict trade that, although there was "No New law against their Importation," resuming the trade "would be Attended with too great hazard"; Cheston to William Randolph, February 18, 1785, CGP.

only nation on earth," protested the Honduran agent in London, "who seem not to know How to dispose of our Criminals."²¹

The failure of Moore's two attempts ended the use of America as a dumping ground for British felons. American opposition was too widespread and prospective schemes to smuggle felons clearly impractical.²² In 1785 a parliamentary committee assigned to resolve the country's worsening penal crisis concluded, "with regret," that the "ports of the United States have been shut against the Importation of Convicts." New sites were feverishly proposed, including Botany Bay in Australia, far removed, as an early proponent emphasized, "from any Part of the Globe inhabited by Europeans." Still, America continued to command nostalgic appeal. "The old System of transporting to America," rhapsodized the committee, "answered every good purpose which could be expected from it."²³

²¹ Robert White to Evan Nepean, January 25, 1785, PRO, Colonial Office Papers 123/3/86; Moore to Treasury, July 13, 1786, HO 42/9/565; Examination of Sec. Nepean, Minutes of Parliamentary Committee on Convicts, April 27, 1785, HO 7/1/23-24; and Oldham, "Transportation of British Convicts," 234-49. In 1784, during the London trial of Charles Peet, one of the mutinous convicts aboard the *Mercury*, the ship's steward testified that the criminals had been destined for Nova Scotia, but Peet asserted that he and others "were to be indented for five years in Virginia." Peet based his claim on papers found aboard the vessel during the uprising. See John Copley, comp., *The Crimes of the First Fleet Convicts* (reprint edn., London, 1982), 213-14. It seems unlikely that the *Mercury* would have sailed to Maryland if Moore had received Salmon's February letter in time, urging that another vessel not be sent. According to James Cheston in early 1785, no convicts again "made their appearance" in Maryland after the arrival of the *Swift*. Cheston to William Randolph, February 18, 1785, CGP.

²² Either just before or soon after the voyage of the *Mercury*, Moore did submit a new proposal to government authorities, offering to transport criminals for a period of ten years. The cost, however, would have been as high as ten guineas per convict, or five guineas per convict if the government had promised to be "answerable for their favorable reception in the United States and for all consequences" resulting from "Attempts of sending them there." Evidently, the proposition was never seriously considered, much less accepted. Proposal by George Moore, n.d., HO 42/5/499.

²³ Report of Parliamentary Committee on Convicts, June 1785, HO 42/6; and Testimony of Sir Joseph Banks, April 1, 1779, *Journals of the House of Commons*, 37 (1779): 311. In later years, only scattered shipments of British convicts, in defiance of both English and U.S. authorities, were unloaded on American shores. The principal culprits were Irish merchants who, with the backing of their government, transported several hundred Irish felons until the late 1780s. Lord Fitz-Gibbon to W. W. Grenville, December 2, 1789, reprinted in Historical Manuscripts Commission, *Thirteenth Report: Fostescue Manuscripts*, appendix, pt. 3, vol. 1 (London, 1892): 546-48; and "An Account of the Sums Paid in the Treasury Office for the Transportation of Convicts within the Four Last Years," [ca. 1790], *Journals of the House of Commons of the Kingdom of Ireland*, 13 (1797): appendix, cccli.

Reviews of Books

GENERAL

HOWARD CLARK KEE. *Miracle in the Early Christian World: A Study in Sociohistorical Method*. New Haven: Yale University Press. 1983. Pp. xi, 320. \$22.50.

The subtitle of this book, *A Study in Sociohistorical Method*, aptly expresses Howard Clark Kee's concern: he is proposing a new historical method for the study of religious phenomena that will avoid the mistake of applying modern values and categories to ancient times. In addition to collecting data pertaining to the ancient world and analyzing that data on chronological, philological, and other lines, it is now necessary for the historian to be aware of the differences between the time he is studying and his own and he must, as Kee states, make an effort "to enter empathetically into the world of a past time, place and outlook" (p. viii). "Facticity" ("Did it really happen?") is a secondary issue; far more important is to determine what a writer who reported an event believed to have occurred. The author has chosen the phenomenon of miracle to demonstrate his views with the hope that the results derived from this slice of history will have implications for other subjects.

The first two chapters consist of a review and criticism of history of the religious methods beginning with Descartes's *Discoveries on Method*, through John Locke, Matthew Tindal, Thomas Chubb, Thomas Morgan, J. A. Turretini, David Hume, and an impressive list of eighteenth-, nineteenth-, and twentieth-century scholars, down to Max Weber, whose "brilliant insights" (p. 57) are reviewed in great detail. After presenting the views of modern scholars, Kee reaches back to the last centuries B.C. and first centuries A.D. to examine "what changing patterns of world-view are evident among pagans, Jews and Christians" in that time period (p. 64). Here the list is regrettably much shorter; we hear of Cicero, very little of Tacitus, Suetonius, Dio, nothing at all of Livy or Virgil, whose views would have been very interesting. Nevertheless, these two chapters alone would make this book worthwhile reading,

but all this is only introduction. After this our attention is turned to the cults of Asklepios (chap. 3) and Isis (chap. 4). With these two examples, the author shows how the social and cultural circumstances of the devotees influenced the meaning of these cults from the earliest times to the second century A.D.

The following one hundred pages (chaps. 5, 6, 7) are devoted to biblical literature, mostly the New Testament. It is here that the author is really in his element, and he succeeds brilliantly in showing how an apocalyptic world view could change a biblical author's approach to his topic and how the Christian tradition could be restated along the lines of the world view of the second century. Furthermore, in the Johannine literature, Philo, Aelius Aristides, and Plutarch, he demonstrates how these writers give a literal interpretation of events on one hand and a symbolic meaning to words and acts on the other. The final chapter deals with "Miracle as Propaganda in Pagan and Christian Romances," that is, the works of Apuleius, Philostratus, Lucian, Celsus, Eusebius, and in the New Testament *Apocrypha*.

The author of this book has already made a name for himself as a foremost New Testament scholar. The present volume represents yet another important contribution from him to the study of early Christianity. Based on solid scholarship, with an orderly and easy style, this book will be used with profit not only by theologians but also by every scholar who is interested in this time period.

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ALAN H. JONES. *Independence and Exegesis: The Study of Early Christianity in the Work of Alfred Loisy (1857–1940), Charles Guignebert (1857–1939), and Maurice Goguel (1880–1955)*. (Beiträge zur Geschichte der Biblischen Exegese, number 26.) Tübingen: J. C. B. Mohr. 1983. Pp. xi, 302. DM 120.

"The independent critic of proven historical compe-

tence will continue to occupy a significant place" (p. 286); thus concludes this study, which in its first words states that it is "intended as a contribution to the history of the interpretation of Christian origins" (p. v). Alan H. Jones begins by putting the work of Alfred Loisy, Charles Guignebert, and Maurice Goguel in a larger antecedent setting: the legacy of Joseph Renan, the Catholic church in France in the late nineteenth and early twentieth centuries and its concern about modernism, the Protestant church and its simultaneous minority status and great influence, ideological and institutional factors, Franco-German cultural relations, contemporary issues in exegesis, and French historiography. The body of the book investigates the work on early Christianity done by the three scholars in question in the light of the claim that their work, in its presuppositions, methodology, and conclusions, was "independent" of a particular church or ecclesiastical tradition within Christianity.

Jones's description of the larger setting in which the three applied historico-critical methods to the early Christian documents is an intelligent and balanced overview. It does not (nor seemingly was it intended to) break new ground in the understanding of the religious, political, cultural, and intellectual circumstances of France at that time. That part of the book relies on other studies that are well chosen. It has the advantages of an overview along with some of its concomitant disadvantages, such as the lack of opportunity to deal with nuances in several instances. The author recognizes the "difficulties in designing an 'independent sector' over against prevailing Catholicism and the small but influential Protestant group" (p. 42). Those difficulties are never entirely overcome in the following and major portions of the study itself.

One chapter each treats the work of Loisy, Guignebert, and Goguel on early Christianity. These parts of the book are extremely detailed and carefully presented. Each chapter gives personal background material on the author as he came to his work on early Christianity and each then deals with the particulars of that work as they appear in his writings. This does not in itself make for easy reading, and along with admirable scholarly care comes dissertation prose.

After reviewing separately the work of the three critics, Jones looks at their admittedly limited interaction and compares the results of their work on five topics: the rise of Resurrection faith, Stephen and the Hellenists, Paul, early Christian worship, and doctrinal diversity and conformity. The very title of the last section concludes with a question mark: "An 'independent' contribution?" (p. 281). It is not clear that this "French tradition of disinterested research into Christian origins" (p. vii) is really fulfilled in the three historians in question. Can

there be such completely "disinterested" research, at least from a Western world so deeply marked by Christianity? This study will be of value to scholars and libraries involved with the history of Christianity, studies in the New Testament, and the religious and intellectual history of France.

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M. L. BUSH. *The European Nobility*. Volume 1, *Noble Privilege*. New York: Holmes and Meier. 1983. Pp. ix, 294. \$35.00.

Now that the once-mighty European nobility has been reduced to a powerless remnant, objective studies of that institution have proliferated. Of the nearly five hundred titles in M. L. Bush's bibliography, fewer than twenty predate 1945. All of these monographs, and the many more not included in the bibliography, are limited to the nobility of a single state or district or to a restricted time span. Bush has taken on the imposing and difficult task of writing the history of the entire European nobility from the Middle Ages into the early twentieth century. As he points out in his introduction, his purpose is to provide not a definitive study, admittedly impossible, but a frame of reference for studies of national or regional nobilities at particular times. He has succeeded admirably in doing just that.

As its title indicates, this study, the first of a planned two-volume work, deals with the privileges enjoyed to a greater or lesser extent by the nobility of the states of Europe. The principal ones included exemption from taxes and other fiscal immunities; the right of political participation through estates and parliaments; the exclusive or nearly exclusive tenure of high offices; honorific rights such as titles, special forms of address, exclusive membership in military and chivalric orders and the like; freedom from certain judicial and service obligations imposed by the sovereign or by private landlords; the exclusive right to landownership and commercial and trading concessions; and a monopoly on hunting. The author devotes a chapter or a section of a chapter to each of these privileges, going from country to country across Europe and over the centuries.

Bush views noble privilege as unjust, divisive, and exploitative. Yet he suggests that the privileges of the European nobility contributed in an important manner to the emergence of the modern democratic state. For instance, he claims that nobles, in return for privileges, cooperated with absolute monarchs in the creation of such modern institutions as the professional bureaucracy and the standing army and so contributed to the process of political mod-

ernization. He asserts that the state was able to introduce regular, direct taxation because fiscal immunities cushioned the nobility from its impact. Privileged noble assemblies prepared the way for popular representative assemblies. Noble privileges, by establishing the principle that the crown's authority should rest on the consent of the subject, laid the foundation for the establishment of representative government and the extension of equal civil rights to the commonalty. These and similar suggestions, though not explored by Bush, have greater or lesser accuracy. But the author overreaches when he asserts that the nobility contributed to the political democratization of society by mistreating their peasants. The nobles' abuse of their seigniorial privileges, says Bush, persuaded their peasants to join the revolutions that introduced democratic principles into European society.

Reading this book is difficult because the text shifts constantly from one country or one century to another. That seems to be characteristic of institutional histories that transcend national boundaries but that must base themselves on national experiences. The difficulty is compounded by Bush's parsimony with words, his severe style, and his decision to omit references to individual nobles and to what he calls "their foibles and eccentricities." It is history without people. Nonetheless, it is a valuable book that rewards the slow and careful reading that it merits.

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JEROME FRIEDMAN. *The Most Ancient Testimony: Sixteenth-Century Christian-Hebraica in the Age of Renaissance Nostalgia*. Athens: Ohio University Press. 1983. Pp. 278. \$24.95.

Jerome Friedman has written an intelligent and engaging book that he calls "the first systematic-functional analysis of Christian-Hebraica during the first half of the sixteenth century" (p. 5). He defines Christian Hebraica as "the use of Hebrew, rabbinic or cabalistic sources for Christian religious purposes" (p. 1), which, he argues, exerted a profound influence on a variety of Christian scholars of this era. This resurgence of Christian interest in Jewish thought, Friedman points out, contributed to the retranslation of the Hebrew Bible, the reinterpretation of the New Testament, and the beginning of a reexamination of Judaism's relationship with Christianity among certain adherents of both faiths.

The greatest virtue of *The Most Ancient Testimony* is its consideration of the evolution of Christian Hebraica in the larger context of the religious and intellectual world of the Reformation. By considering a variety of Protestant Hebraists, especially

Michael Servetus (the subject of Friedman's earlier biography), Johannes Reuchlin, Paul Fagius, Sebastian Münster, and a circle of scholars associated with Luther, Friedman ably demonstrates the heterogeneity of their motivations and Hebrew sources. The gamut runs from Servetus's radical critique of Trinitarian Christianity, to Reuchlin's "mystical nostalgia," and to Fagius's fascination with the Pharisaic context of Jesus' teaching. Friedman discusses separately both the theological justifications and the scriptural hermeneutics of each of his major subjects. He also devotes an interesting chapter to the missionary writings of the Christian Hebraists, which were composed, he suggests, as political guises behind which they were able to carry out their own Judaic scholarship.

Friedman concludes that Christian Hebraica constituted a variation on the pervasive theme of nostalgia for a previous age that informed both the Renaissance and the Reformation. Rather than an isolated phenomenon, Christian Hebraica should be considered together with Italian humanism, ancient theology, and Reformation scripturalism as an expression of a yearning for a more innocent past by Christians in a state of spiritual crisis over the meaning of their own beliefs and practices.

Friedman's book, however, is not without certain flaws. He emphasizes the need to integrate both Hebraic and Reformation, and Renaissance and Reformation studies. Unfortunately, on both counts, his book is less than satisfactory. The author is insufficiently informed about some basic issues of Jewish history, Judeo-Christian relations, and Christian study of Judaism prior to the sixteenth century. He makes a number of embarrassing factual errors (for example, the number of Jews killed during the Crusades [p. 17]; the *monte di pietà* replacing Jewish pawnshops [p. 17]; "continual book burning" [p. 18] and more) and Hebrew mistakes (for example, *Mikneh*, not *Mikveh Abraham* [pp. 21, 38]; *m'chokakim*, not *m'chochakim* [p. 229], and elsewhere). He seems unaware of the voluminous literature on Judaism written by Christians, especially Jewish apostates from the thirteenth to sixteenth centuries, and especially the prolonged attempts by these converts to locate Christian meaning within Jewish texts long before the Reformation.

His treatment of Jewish culture and Jewish-Christian liaisons in the Renaissance is superficial and constitutes the most serious weakness of the entire book. Friedman underestimates, to my mind, the seminal role of Pico della Mirandola, Cornelius Agrippa, and others in the emergence of Christian Hebraica, particularly Kabbala. His lack of consideration of Reuchlin's great debt to Pico is especially noticeable. Thanks to the work of Chaim Wirshubsky and Moshe Idel on Pico's Jewish associates, we now understand more precisely the process of

transmission of Jewish sources into Christianity. Friedman does not adequately demonstrate the Hebraic and cabalistic links between Florentine ancient theology, Catholic reform (particularly its concern with the Jewish mission), and the Protestant Reformation, despite his use of "Renaissance" in the title of his book and his final plea for the integration of these three distinct strands. Friedman also underestimates the relation of magic and hermeticism to Christian Hebraism. He seems less interested in understanding the use of Kabbala among Christians than in their simpler exegetical and philological concerns.

These criticisms are not intended to diminish the importance of this book and its original contribution to the subject of Christian Hebraica. The task of bridging the gap between Judaic and Christian studies in the era of the Renaissance and Reformation (and in other periods) is a formidable one and Friedman succeeds better than most of his predecessors. Future work done by both historians of Jewish and Christian culture in this period will undoubtedly rely on and further refine his pioneering reconstruction.

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STEVEN OZMENT. *When Fathers Ruled: Family Life in Reformation Europe*. (Studies in Cultural History.) Cambridge: Harvard University Press. 1983. Pp. viii, 238. \$17.50.

Every few years scholarly books on the Reformation are written in this country that challenge established theories, present fresh insights, and stimulate our research. In a sense they are milestones in the field of Reformation studies. I think that Steven Ozment's well-researched book on "Family life in Reformation Europe" will also be recognized as such a work. Ozment uses the theological literature and a substantial number of monographs, but what sets his book apart and gives it a particular charm is the careful scrutiny of a large body of vernacular literature such as "house father" books, guides to estate management, pamphlets, chronicles and diaries, woodcuts, sermons, and catechisms. The book is arranged around four major topics: the defense of marriage against the antifeminism of the late Middle Ages, the roles of husbands and wives, the bearing of children, and the rearing of children. The basic facts are known: the Reformation introduced a new appreciation and elevation of family life. Recently, however, some historians have emphasized the negative aspects of this development: in particular, the harshly paternalistic character of the Protestant family, the new subjection of wives to males' dominance, and the systematic disciplining of

children through intensive indoctrination, if not beatings.

We are discovering now with some amazement that both the theological and popular literature and personal documents of the time, such as diaries, tell a very different story. The father was indeed head of the family. But authority was shared by husband and wife in their different spheres. There was a much greater balance of rights and responsibilities. The picture of the domineering male who was not even moved by the death of his own children has been vastly exaggerated, if it is not a myth altogether. The relations of parents to their children were characterized by warmth and affection, not by cold aloofness and domination. There was indeed much concern with the education of children—not to turn them into docile subjects of absolutist rulers but to educate responsible members of a Christian community. Ozment produces so much evidence from vernacular literature, letters, and diaries that even a skeptical reader will be impressed, although I must admit that Gerald Strauss's well-founded thesis about the intent of Lutheran education is not really refuted. The picture that finally emerges in Ozment's book of family life in Protestant Germany is quite sympathetic. Naturally, one wonders whether family life has not always had its own peculiar atmosphere, regardless of the prevailing religion.

As part of the general thesis Ozment also discusses several other fascinating problems. He traces the evolution of the legal aspects of secret marriage, betrothal, and public marriage in the late Middle Ages and the sixteenth century. There is a section on the workings of the Protestant matrimonial courts, particularly in Zurich. And he outlines the gradual acceptance of grounds for divorce by various Protestant theologians. Of course, much scholarly work has been done on these problems. Still, such a lucid discussion within the framework of a study of marriage in the sixteenth century is quite useful. In short, both professional historians and students will enjoy this well-written book, its fresh and sympathetic approach, and its immensely vivid and colorful detail.

Nevertheless, I am taking the liberty of adding a few critical remarks. Although Ozment makes some references to Anabaptism, he is not fully familiar with the varieties of views the Anabaptists had on marriage and divorce. (There is also a bad mistake on page 86: Anabaptism was not a capital offense in Protestant Württemberg, definitely not by 1571.) These may be minor points. But why did Ozment not use visitation transcripts? Why did he not consult German archives? Ozment does not think much of statistical work, but would it not have been wise to substantiate in some detail his unusual claim that 40 percent of all women were single before the Reformation? There is a lot of talk about sex, but why

does Ozment not tell us about the size of the Protestant family? And would it not have been revealing to consult the codes of civil law on questions such as the property rights of married women, of widows and children? I suppose the answer to these questions is that every historian prefers his own material and methodology. After all, it may be good that there is a diversity of approach. In any case, I think that Ozment's skillful use of hitherto-neglected material and his vivid picture of marriage and family in Protestant Germany will give a new direction to sixteenth-century social history.

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A. LONDON FELL. *Origins of Legislative Sovereignty and the Legislative State*. Volume 1, *Corasius and the Renaissance Systematization of Roman Law*; volume 2, *Classical, Medieval, and Renaissance Foundations of Corasius' Systematic Methodology*. Cambridge, Mass.: Oelgeschlager, Gunn, and Hain or Athenäum, Königstein im Taunus, Federal Republic of Germany. 1983. Pp. xx, 344; xvi, 319.

Jean de Coras, or Corasius (ca. 1515–72), is best known today for his role of *rapporteur* in the famous trial of Arnault Du Tilh that has recently been brought to our attention by an excellent film and historical study, *The Return of Martin Guerre*. In his own day, Corasius was famous in legal circles for his brilliant lectures at the universities of Toulouse and Valence and his numerous publications on legal subjects. His stature is best reflected by the fact that the latter university was willing to pay 1400 livres per year for his services but gave his more famous contemporary Cujas only 600 livres per year a decade or so later. Corasius continued his active career as an author and counselor in the Parlement of Toulouse until he had the misfortune to become a victim in the massacre of Saint Bartholomew. In spite of his voluminous output, Corasius's scholarly contributions have been largely forgotten. Such distinguished scholars as W. F. Church, J. H. Franklin, M. P. Gilmore, D. R. Kelley, and P. Mesnard have managed to produce studies on sixteenth-century constitutional and legal thought without finding the need to mention his contribution. One is therefore surprised to find A. London Fell making Corasius the central figure in the first two volumes of his projected four-volume *Origins of Legislative Sovereignty and the Legislative State*.

In his study Fell seeks to demonstrate "that the modern concept of the state as an autonomous public entity began in the early modern period of European history, and that the basis of that concept of the state was a systematized concept of public

legislation. The term 'legislative state' is used here to characterize modern concepts of the state in which public legislation is the state's main foundation, just as the term 'legislative sovereignty' typifies modern concepts of sovereignty in which public legislation is the principal basis" (1: vii). The two volumes under review deal with Corasius, and the remaining two volumes, if we may judge by the preface and their subtitles, will be devoted to proving that modern ideas of legislation, sovereignty, and the state did not emerge in the Middle Ages, as some medievalists have argued, nor did they spring newborn from the brain of Jean Bodin, as some of his admirers have supposed. Rather they were Corasius's creation.

Corasius was a prolific writer, but to Fell he did not make his great contribution to the concept of legislative sovereignty and the legislative state until he published *De juris arte libellus* in 1560. In this work Corasius used Aristotle's four questions and four causes to provide the essential structure and organization. When Corasius asked what was the efficient cause of law, it was but natural that he was led to answer that it was the power to make law or the lawmaker. This in turn "resulted in a conception of the sovereignty of the legislator" (1: 77). "There is nothing," Corasius declared, "so great or so high as the power to make law" (1: 86). Late medieval and early Renaissance jurists, Fell points out, had "dealt with sovereignty from the perspective of jurisdiction" (1: 98). Furthermore, Corasius clearly distinguished between legislation and custom. Custom, he asserted, does not have the force of law unless it is approved by the legislator. The final and most powerful cause to Corasius, "is the state itself [*res publica ipsa*] and the civil society of men" (1: 179). Thus the state became an independent entity.

About a third of the second volume consists of an investigation of classical, medieval, and Renaissance scholars to see what contribution, if any, they made to Corasius's methodology and theory of legislation. The second third deals with Corasius's "early development as a systematist interested in art and legislation" (2: viii). The final third consists of texts and documents. Perhaps the most interesting aspect of this volume is Fell's conclusion that "Corasius' focus in the *Ars* [*De juris arte libellus*] on the four causes and four questions was not influenced by Ramus" (2: 184).

This is a difficult book to read. It is poorly organized and repetitious. Aspects of Corasius's life appear in sections in both volumes as do explanations of Aristotle's four causes. In spite of these reservations, there is no doubt that this is a major contribution. Fell is immensely learned. He has rescued Corasius from oblivion and he has provided valuable analyses of many little-known late medieval and Renaissance jurists. It is necessary to await the

appearance of the next two volumes before making a final judgment, but it is probable that he will succeed in dethroning Bodin and establishing Corasius as the father of the idea of legislative sovereignty and the legislative state.

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JOHN LANGDON BROOKS. *Just before the Origin: Alfred Russel Wallace's Theory of Evolution*. New York: Columbia University Press. 1984. Pp. xiii, 284. \$30.00.

With the rise of the Darwin industry in the past fifteen years, it is thoroughly appropriate that historians and biologists reexamine Alfred Russel Wallace's contribution to the theory of evolution by natural selection. John Langdon Brooks is the division director of Biotic Systems and Resources at the National Science Foundation. *Just before the Origin* focuses almost exclusively on the details of Wallace's two adventures to the tropics. The first, taken with Henry Walter Bates to the Amazon, lasted from 1845 to 1852; the second, a solo venture to the Malay Archipelago, occupied Wallace from 1854 to 1862. The body of this book describes Wallace's discoveries in natural history and his attempts to generalize about the flora and fauna that he explored.

Brooks surveys almost exactly the same terrain that H. Lewis McKinney did in his *Wallace and Natural Selection* of 1972. Where McKinney better placed Wallace's accomplishments within the context of the contemporary research of other mid-century naturalists, Brooks gives us an account that is more informative about the varied biological material of the tropics. As an ecologist by training, the author understands the problem of tropical speciation, and he successfully presents this in nineteenth-century terms, so that we gain a real appreciation for Wallace as a first-rate naturalist.

But Brooks supplements McKinney's account in a more important way. McKinney explored the relationship between Wallace and Darwin at two significant moments. He showed that Wallace's first evolutionary publication, "On the Law Which Has Regulated the Introduction of New Species" (1855), was carefully studied by Darwin. McKinney further claimed that Darwin must have held the famous letter of 1858, in which Wallace described the mechanism of natural selection, for two weeks before turning it over to Charles Lyell and J. D. Hooker. This conclusion went contrary to the standard accounts and raised the possibility that Darwin failed to treat Wallace as fairly as recent Darwinian studies have claimed. In this new account Brooks goes further. Through a study of the records of the packet ships plying between the Malay Archipelago

and England and a reexamination of Darwin's manuscript on "Natural Selection," Brooks concludes that Darwin not only deceived Wallace and the rest of the world about the exact sequence of events but also spent the weeks he expunged from the historical record revising his account of the divergence of species. The charge is suggestive but not convincing. It goes against what we know about Darwin's intellectual development in the mid-1850s, against the fundamental differences between Darwin's and Wallace's notions of divergence, and against Wallace's own ready acceptance of the traditional account.

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STEVEN SEIDMAN. *Liberalism and the Origins of European Social Theory*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xii, 419. \$35.00.

When sociologists seek to ground contemporary practice in classic social theory, their view of the past is bound to reflect present needs and interests. That Steven Seidman approaches Karl Marx, Emile Durkheim, and Max Weber in an attempt to root his discipline in what all three had in common suggests a good deal about his project. The liberalism of his title is a radical liberalism, determined to hold liberal thought and action close to the emancipatory promise of its origins. Believing that his three thinkers (and the Enlightenment tradition on which they drew) exemplified this project, he writes against several tendencies in the history of his subject: (1) the notion, associated with Robert Nisbet, that the origins of sociology lay in a conservative reaction against the Enlightenment; (2) the related view that Marxism and the academic tradition rooted in Durkheim and Weber are fundamentally separate and opposed; and (3) those histories of social theory that ignore either its analytic or its "non-empirical" (moral, political, and metaphysical) components, seek to reduce it either to simple ideology or to pure positivist science. The last point outlines a "multidimensional" perspective Seidman shares with recent work by Jeffrey Alexander. The result is a determined and vigorous essay, quick to engage in argument with other writers and offering some perspectives of considerable interest. Unfortunately, the result is diminished by a style so full of first-person pronouns that establishing the author's presence in the debate sometimes seems the most pressing task. Moreover, his attempt to bring all three of his major subjects together sometimes obscures what was distinctive in each one.

Readers may be most surprised by the attempt to associate Marx with an essentially liberal perspective. But Seidman emphasizes that what distin-

guished Marx from other opponents of bourgeois society in his time was his uncompromising modernism, his insistence that the transformation of society had to arise out of its own existing tendencies. Marx wanted to realize possibilities created by liberal society, and he rejected any return to earlier modes of community. The point is not altogether novel, but Seidman is right that it deserves to be underlined. Nonetheless, his attempt to make Marx's liberal inspiration central throughout his whole career leads to problems. Seidman gets tangled up in the contradictions of Marx's attitude toward subjective autonomy, writing at one point that "Marx never abandoned the idealist notion of subjectivity as a free moral agency" (p. 94) and later that he displayed "a tendency to view, in a fairly simplistic way, the subjective development of the proletariat as a mere appendage to material progress" (p. 126). The notion that Marx's opposition to bourgeois liberalism was total "only during those periods when bourgeois society suppressed its liberal principles" (p. 86) seems pointless given Marx's conviction that such suppression was built into the deep structures of capitalism.

Turning to Durkheim, Seidman grounds his case in what he sees as the impact of a native French revolutionary tradition on his thinking. This argument is hard to credit, since it is carried out largely on the plane of homologies or parallels, sometimes vaguely defined. More important, we never learn here about Durkheim's criticisms (in "The Principles of 1789 and Sociology") of the revolution's form of individualism as abstract and divisive. The Weber section is even more questionable. Much of it rests on a reading of *The Protestant Ethic and the Spirit of Capitalism* as an attempt to give a "piercing intellectual jolt" to German liberals, bringing them back to the energies stored up in the Calvinist, as opposed to the Lutheran, tradition. This reading of Weber's essay (not unique to Seidman) seems to me altogether untenable. It denies that Weber's interest in his subject in 1905 was already shaped by the conviction that the original emancipatory power of Western rationality had become a component of the "steel-hard housing" or "iron cage" that hemmed in individual action in his own time. Seidman seems to want to support his reading through a critique of some who have made rationalization the central element of Weber's work. I can only say here that none of this argument reduces the importance of the *stahlhartes Gehäuse* image in the 1905 edition of *The Protestant Ethic*. Seidman's overall presentation of Weber, although properly emphasizing the scope for moral responsibility offered by modern conditions, fails to convey the deep pessimism or the recurring spirit of *demnoch* (manly facing up to a bleak existence) that was so characteristic of him after his breakdown. To associate Weber with liber-

alism is entirely correct, of course. But Seidman's manner of doing so squeezes the juice of desperation out of Weber's tormented consciousness, drying up his attraction for extreme positions.

Seidman's basic contention, that his three figures all shared an underlying desire to fuse liberalism and revolution, is correct on one level and deserves to be taken seriously. But those who have written on the subject before now have not been so blind to this project as he implies. What he misses is the crucial point: that everything depends on how the fusion is made—to what end and with what attitude toward the residues of liberal or revolutionary commitment that resist such combination? Only from this perspective do the classic thinkers on which he focuses begin to reveal their particular individual personalities.

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PHILIP ABRAMS. *Historical Sociology*. Ithaca: Cornell University Press. 1982. Pp. xviii, 353. Cloth \$29.95, paper \$11.95.

Philip Abrams is genial and persuasive in arguing that history must be sociological, and sociology historical, if they are to pursue their respective missions well. The concern for linkages between these disciplines has been growing, from Frederick Teggert's *Theory and Process of History* (1941) through Bernard Stern's *Historical Sociology* (1959) and Werner J. Cahman and Alvin Boskoff's *Sociology and History* (1964) to Richard H. Brown and Stanford Lyman's *History, Structure, and Consciousness* (1978) and Peter Burke's *Sociology and History* (1980).

Abrams's book is an important addition to this discourse across tribal boundaries. On one side of these boundaries, sociologists have been much concerned with social change, sometimes focusing on the development of institutional areas such as industrial capitalism or the nation-state, sometimes on the emergence of "total society" or the "world system," as in Talcott Parsons or Immanuel Wallerstein. On the other side of these boundaries historians have become more quantitative in their research techniques and have begun to study hitherto-unconventional matters such as oppression, crime, family life, dying, madness, and sexuality. This has resulted in poaching by each tribe in the other's forest, so much so that it is now unclear as to what game belongs to whom.

Abrams argues that the two tribes really are one, for both are dedicated to unraveling the puzzling relationship between human agency and social-historical constraints. How do large-scale social transformations occur—how do individuals shape and

become shaped by them? Today this question guides both historians and sociologists.

With great erudition in both historical sociology as well as social history, Abrams insists that there in fact is "no relation *between* them because, in terms of their fundamental preoccupations, history and sociology are and always have been the same thing" (p. x). Abrams's defense of this assertion is not through philosophic analysis of concepts or presuppositions. Instead, he proceeds inductively by closely examining works of major historical sociologists and then offering these as a gift to historians who have not yet understood that they themselves are (or should be) doing sociology too. In the same large and critical spirit, however, Abrams then addresses sociologists who are not historically oriented, such as students of deviance, social psychology, and the like; he explains that to do properly what they do, they really are (or must become) historians as well.

The first three chapters of *Historical Sociology* are devoted to the theories of Karl Marx, Max Weber, and Emile Durkheim concerning the development of industrialism. Subsequent chapters consider Parsons and the debate on "convergence" between the U.S. and the USSR; the formation of "states"; the idea of the "event" as a legitimate concern of history and sociology; individuals and sociological generations; deviance and revolution; and a final chapter on the limits of historical sociology.

Abrams seeks to bridge the gap between the two by denying that it exists. Sociologists and historians have not only been poaching in each other's forests—they also have been mating there to such an extent that their offspring now form the dominant bloodline. Now let us legitimate the bastards, Abrams seems to be saying. This is a generous intention, but I question under what code of law such legitimation will take place. For the code that Abrams presupposes is at bottom neopositivist—a nomothetic notion of science with sociologists providing most of the covering laws and historians providing most of the cases.

Abrams sees analytic theoretical historiography as virtually identical to comparative historical sociology, since they both constitute, in Weber's words, "an empirical science of concrete reality" (p. 82). But such an experimentalist conception of historical or social science implicitly defines reality and knowledge in terms of a factual world governed by nomothetic causal laws. These assumptions are not contained in historical evidence itself, nor can they be apodictically grounded. Instead, this mechanistic metaphor, much like a Kantian regulative principle, functions to *constitute* a subject matter.

There is more both to sociology and to history than this, however; there are more ways than neopositivism for constituting a field for sociological or historiographical practice. And, thus, there are

broader bases available for possible integrations between these disciplines than Abrams conceives. For example, structuralist social historians, such as those of the *Annales* school, constitute their subject matter as a system of synchronous homologues and transformations, much as do structural anthropologists such as Claude Lévi-Strauss or Pierre Bourdieu. But though Abrams mentions with admiration the *topics* of these historians, there is little space in his conceptions of sociology-history for their logic of method.

Similarly, Abrams subordinates narrative history to historical sociology and analytic historiography. The essential data for this historical science are to be defined through controlled comparative analysis on the experimentalist model. What, then, is the role of hermeneutics? And what can be the epistemological status of narrative, either in historical writing or in the case studies and ethnographies of social scientists? Abrams does not discuss hermeneutics, and he dismisses the teleological thinking of narratives as based on "metaphors that lead one away from a genuine historical sociology rather than toward it" (p. 6). Thus Abrams rejects teleological thinking as metaphoric, only to replace it with the metaphor of society-history as a machine.

These perhaps are criticisms by a philosopher more than by a working historical scientist; they are criticisms of Abrams for distinctions not drawn and a range of practice not covered. On his own ground, however, Abrams is formidable—a sociologist who claims his tribe has been historians all along, he seductively invites both sociologists *and* historians to accept their new fraternity.

RICHARD HARVEY BROWN
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REUVEN BRENNER. *History—The Human Gamble*. Chicago: University of Chicago Press. 1983. Pp. xiv. 247. \$17.50.

Not every slim book offers "a theory of history," moreover, "a general theory of human behavior . . . which makes predictions about a wide variety of human activities, which is supported by the empirical evidence (statistical and nonstatistical), and which seems to shed light on various historical events that no other approach can" (p. 198). As one might expect, such a labor is morally inspired. "In contrast to my uniform approach to all aspects of human behavior, even a superficial look at various disciplines in the social sciences reveals a human being who is schizophrenic" (p. 198). With his theory, Reuven Brenner hopes to help restore today's fragmented self and also to "re-center" it (my word), so that it has at its disposal all the voluntaristic skills and choices Milton Friedman celebrates. (This

makes sense, since Brenner studied at the University of Chicago after taking degrees at Hebrew University.) As an economist, he is unusual and refreshing, for he discounts elegant mathematical models that explain nothing, since they originate in premises about human action that everyone agrees are purely utopian. And, unlike American economists, he is an intellectual: he reads several languages, appreciates history, is philosophically alert, and—most important—carries into his work a humanist sympathy and vision alien to all economics chained to “science” and “value-freedom.” As a citizen of Israel and France, a teacher in Canada, and a student of world history, his relativism comes ready-made, so his value choices are intellectually created rather than simply absorbed, uncritically, as with so many social scientists.

The problem with the book comes, however, in Brenner qua historian, anthropologist, sociologist, psychologist, political scientist, and philosopher of culture—all of which he must become, given his ambitions. Because he cannot fully relinquish central notions of economic “theorizing”—especially that people “choose” this or that action in rational response to events around them—he does not measure up very well as a general social scientist, à la Max Weber, for instance (whom he foolishly believes he shatters between pages 121 and 123!). The book is meant to “prove” a simple thesis, repeated dozens of times: people above all desire “self-preservation,” and when they experience a sudden fluctuation in their condition relative to others, they “gamble” on new ideas, that is, they “think” or behave “creatively.” His “proofs” range from simple utility functions for gambling on lotteries to analyses (sometimes quantified) of medieval and prehistorical existences, the invention of language, anti-Semitism, the Protestant ethic, and so on. His a priori assumptions—“the perception of *inequality* makes people what they are (that is, *thinking* creatures) and . . . markets, legal institutions, and literacy emerge in response to increases in human populations” (p. xi, original emphases)—would be faulted, as he knows, by gigantic stretches of social-science research. Yet he claims he has advanced knowledge (not just for economics) by presenting a “concise, rigorous,” and “testable” model of human action, despite its gaping substantive sacrifices.

His data for medieval life come mostly from Carlo Cipolla and Marc Bloch (both distinctly uninterested in the nonmaterial), for prehistory from Jack Goody and Mark Cohen, for global history from a minor Toynbee work, and, for usury, from Benjamin Nelson. His reading of these sources is highly selective and elliptical; he is hunting for substantiation of a faith, not for careful understanding of an epoch or subject. Of countervailing explanations very little is heard, unless by caricature. Brenner’s idea is

comforting—that people, when pressed, can sometimes create solutions by dint of ingenuity and diligent application—but he must omit or ignore so much of the cultural-historical record that his claim to have devised a general theory of human action is completely overstated. He will teach his fellow economists some things, but the *Geisteswissenschaften* will view his book as anachronism, an expression of rationalist faith, and not as cogent analysis.

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DAVID GOODMAN and MICHAEL REDCLIFT. *From Peasant to Proletarian: Capitalist Development and Agrarian Transitions*. New York: St. Martin’s. 1982. Pp. xii, 244. \$27.50.

For several millennia people have worked on the land, and over the course of time they have moved from agriculture into other kinds of jobs. It is singularly unfortunate that the terms used to describe the men and women in this process have hardened in academic English into *peasant* and *proletarian*. No two words have more bedeviled attempts to understand the modern world than these. Spanish *campesino* is straightforward, covers a multitude of rural occupations, and does not raise one’s theoretical hackles; nor does the German *Bauer*, which, moreover, even carries a suggestion of positive values. In English, however, peasant is used where it certainly should not be—much of Latin America—and not where it probably should be—say, Kansas and Nebraska up to 1940. Although you know a Mexican *campesino* when you see one, no one can agree on who is or is not a peasant in Mexico.

If the terms are artificial in the homeland of capitalism, they become positively befuddling in such places as Latin America, where capitalism is not an organic growth but brought from Europe, either in sixteenth-century galleons or perhaps nineteenth-century steamers—even this is not clear. It is no small part of the entire syndrome of dependency that terms such as peasant and proletarian, and more recently and more implausibly, *Junker* and *farmer* are applied and accepted in Latin America when there are perfectly good words, rooted in the region’s own historical experience, that are available.

David Goodman and Michael Redclift begin their book with a stroll through the familiar landscape of Marxist literature on the agrarian transition in Europe, its refinements by Karl Kautsky, its challenge by Aleksandr V. Chayanov, and a brief description of changes in European agrarian structure since 1945. The discussion then leads through the equally familiar but big debates on circulation and produc-

tion, terms of trade, dependency, articulation of modes of production, and the agricultural surplus (centered on the work of I. Wallerstein, A. G. Frank, E. Laclau, S. Amin, and many others). By the end of chapter 4, the reader has been given a well-organized, lucid summary of a great deal of recent writing. The aim of these pages is to set the theoretical stage for case studies of modern Brazil and Mexico in the subsequent two chapters. In a recent and roughly parallel book, Alain de Janvry (*The Agrarian Question and Reformism in Latin America* [1981]) reviews much of the same material but out of it constructs his own interpretive synthesis; that is not the case here, where the authors strive not for originality but review. If one accepts the usefulness of the exercise, it is a task well done.

The case studies are also able and up-to-date summaries of material and debate. Although no one agrees on what to call Mexican rural folk, there is no doubt that the continuous and unilinear transition to wage labor envisioned by Marx did not take place in the Mexican countryside. Rather, the reverse held true, at least for a period after 1934, when the state, in part to build up a political constituency, created a "neo-peasantry" through the *ejido* program, which dropped the percentage of landless laborers from 68 to 36 percent of the rural work force (p. 190). The authors are sensitive to the problems of definition (that is, when does a peasant cease to be a peasant?) but, keeping to the high ground of overview and theory, leave the issue unresolved. By way of contrast, we have in the recent work by Florencia Mallon (a book not available to Goodman and Redclift) a demonstration, in persuasive and unusual on-the-ground detail, of the halting, discontinuous, and violent path to capitalism in highland Peru. It is precisely the kind of work that Goodman and Redclift call for and bears out their contention that "in place of one agrarian transition . . . we are faced with several" (p. 217). Their own work helps clarify and focus certain elements of this important question.

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DONALD M. LOWE. *History of Bourgeois Perception*. Reprint. Chicago: University of Chicago Press. 1983. Pp. ix, 206. \$6.95.

The history of "bourgeois" perception is an interesting idea, and the project of synthesizing Marxism and phenomenology could be an exciting enterprise. But Donald M. Lowe fails to achieve these high aims. This book is more of a gloss than a useful study of changing perceptions. The prose is on such a level of abstraction that it is a sparse overview with

tiny snippets of cultural landmarks incorporated by reference. One has the feeling of racing along a temporal narrative at breakneck speed and occasionally noting a milestone (Auguste Comte, Charles Dickens, George Eliot, Gustave Flaubert, Karl Marx, J. S. Mill, Sigmund Freud, Marcel Proust). We are given a sentence or a paragraph or two of bland description, never enough to develop a case. If one knows anything at all about any of the figures Lowe takes up, one sees just how superficial the treatment is.

This quality of a synoptic outline is reinforced by the too frequent use of the *Encyclopaedia Britannica* and other compendia as sources (see p. 188, nn. 5, 7). Lowe writes about James Joyce but does not cite him; rather, he relies on Hugh Kenner as cited in Sharon Spencer. He does not work with the original of Oswald Spengler, but Spengler as interpreted by H. Stuart Hughes (p. 116). We do not read Adam Smith as he said it, but as he is seen by R. Meek and A. Skinner and cited by Gary Wills in the *New York Review of Books* (p. 178, n. 9). This disconcerting quality of third-level redigestion of thought is a pity, because Lowe obviously has read and assimilated a great deal of material. Intellectual historians, however, will wish to have their interpretations based on the original texts or the best possible reprinting of them.

Lowe adopts the Frankfurt School culture critique that "the contemporary person has much less of an integrated personality, much less of an inside" (p. 14) than pre-1915 individuals. To substantiate this conception Lowe needs much better tools than he has to get "inside" modern historical man. Many readers will doubt that "people mean exactly what they say" (p. 19).

Lowe presumes to point "to a new problem in historical method, namely, that of reflexivity in historical consciousness"—by which he means we must try to view the events of the past as much as possible in the way the actors of the past experienced them. Thus, Lowe states, "We need to show how the various groups of French people in 1789 saw their unfolding revolution, and in our explanation of it we should not reduce the significance of their experiences" (p. 175). In other words, historicism. There is nothing here that would not have been entirely acceptable to Wilhelm Dilthey and R. G. Collingwood. With "reflexivity" Lowe restates the historian's traditional epistemological problem of empathy with actors in past concrete historical situations in phenomenological language and presumes to have told us something new.

There is, of course, a major problem with generalizing about "bourgeois" anything at any time. The questions immediately arise: which strata and subgroups of the bourgeoisie, when, and where? But we should not cavil at Lowe with objections calling

for temporal, social, and geographical precision. There is a place in historical writing for high synthesis from the Middle Ages to the present in the compass of a small volume. Lowe would have done better to take three or four pivotal examples and elaborate them in depth rather than offering us this very abstracted and condensed outline. Then he could have shown us the modes of culture, symbol formation, and relativized perceptions in historical context. What he gives us in this presentation is a narrative of the presumed results of changes, without showing why or how anything changed. As it stands, this "history" is so thin it could not serve as a satisfactory textbook for intellectual history courses.

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D. C. M. PLATT. *Foreign Finance in Continental Europe and the United States, 1815–1870: Quantities, Origins, Functions, and Distribution*. Boston: George Allen and Unwin. 1984. Pp. viii, 216. \$27.50.

This is a useful book with a strong point of view. D. C. M. Platt, as the jacket flyleaf emphasizes, is opposed to historical analysis that stresses imperialism and dependency. To him, the sources of national economic growth are largely domestic. The critical provision of capital comes not from inflows but from domestic savings, including plowed-back profits. His account of the movement of foreign finance into France, Russia, Austria, and the United States between 1815 and the Great Depression hammers away at the thesis that most economic historians are mistaken in focusing on new issues of foreign securities, since a large portion of those issues promoted by foreign underwriters are initially bought, or subsequently bought back, by investors of the borrowing country. The foreign underwriter and his speculating countrymen buying new issues on credit expect to sell them; they perform a useful service in encouraging domestic investors in the borrowing country to part with their savings, but they do not place much capital on a long-term basis. "Securities flit from one market to another" (quoting Charles A. Conant, writing in 1899 [p. 109]).

Platt scores heavily in correcting downward estimates of foreign investment for various countries made by Rondo Cameron, Herbert Feis, Bertrand Gille, C. K. Hobson, Leland Jenks, Alan Milward and S. B. Saul, and many others, although one finds his characterization of these estimates as "mistaken," "wrong," "absurd," "glib and implausible," and so forth a little schoolmasterish and wearing (part of this feeling arises from my comparable guilt in a book that appeared too late to be taken to task). But Platt has his own doubts, both as to the possible

strategic if marginal character of foreign investment in eliciting domestic attention and enthusiasm in the borrowing country and as to whether his generalization applies to British lending to India in the 1850s; to Argentina, Australia, and South Africa from 1885 to 1890; to Canada in the decade to 1913; or to French lending to Russia from 1887 to 1913.

The book mines the correspondence books of the Baring Brothers for contemporary pictures both of new issues and of trade in outstanding securities. It is not, however, a business history of Baring in the way that Martin Buist's *At Spes Non Fracta* (1974) is of Hope of Amsterdam. The bulk of the material comes from secondary sources and contemporary accounts such as *The Economist*, mostly in English and French, plus some Spanish, but no German, Russian, Dutch, or Italian. One notable absence in the excellent fifteen-page bibliography is Matthew Simon's *Cyclical Fluctuations and the International Capital Movements, 1865–1897* (1978). The two periods overlap by only six years, to be sure, but Simon is an author who works intently at trade in existing securities and has a point of view regarding the surplus of European capital for export and its importance to the growth of the United States (from 1863 to 1873) that is very different from that of Platt.

In such matters as "*dependencia*," perceptions may matter as much as the arithmetic. Franco Bonelli, for example, calls Italy a financial "colony" (*La crisis del 1907* [1971], p. 43) and the financial dependence of Spain through 1868 was surely greater than that of Italy. When the Paris market stiffened in 1866, 1889–93, and again in 1907, Italy, at the end of the line, was brought up short and felt convulsions. One can be grateful to Platt for his penetrating analysis of numerical overestimates of data on foreign lending, based on new issues, without necessarily following him all the way in his dismissal of their importance.

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HARTMUT KAEUBLE. *Soziale Mobilität und Chancengleichheit im 19. und 20. Jahrhundert: Deutschland im internationalen Vergleich*. (Kritische Studien zur Geschichtswissenschaft, number 55.) Göttingen: Vandenhoeck und Ruprecht. 1983. Pp. 322. DM 72.

Hartmut Kaelble has assembled in a single volume a collection of articles that have formerly appeared in various journals and anthologies on both sides of the Atlantic. An initial section deals with upward social mobility and educational opportunity in Germany from 1850 to 1860. A second section, subsumed under the heading "Germany in International Comparison in the Nineteenth and Twentieth Centu-

ries," explores urban mobility and the recruitment of entrepreneurs in Europe and the U.S., the emergence of a West European pattern of educational opportunity, and the possibility of societal convergence between Germany and France. The glue supposedly binding these articles together is a differentiated developmental model, set forth in the first chapter, that relates social mobility and equality of opportunity to the changes in occupational, social, demographic, and political structures that have accompanied the stages of industrialization.

As Kaelble is the first to admit, such a social-historical model is difficult to test because of gaps and inconsistencies in historical research. His tentative conclusions are meant to be hypotheses for further, detailed study. Although social opportunity and mobility in Germany gradually increased during the nineteenth and twentieth centuries, such improvement seems to be due more to political than to economic developments. Research in local history and in the history of specific occupations and groups, particularly the petite bourgeoisie, would tell us more about the relationship of both economic and political factors to social mobility. Using a small variety of urban studies, Kaelble cautiously disproves Alexis de Tocqueville's claim that movement between the classes was much more fluid in the U.S. than in Europe. More interregional research could verify that similar types of cities had similar social mobility, that frontier cities had more mobility than established ones, and that economically stagnating regions provided less opportunity for improvements in social status. Additional work could also better explain why some European countries, such as England and Sweden, took the lead in providing more educational opportunity for the lower classes, while others, such as Holland, lagged behind. And, lastly, data relating to economic growth suggest that, although France and Germany had dissimilar mobility patterns in the nineteenth and early twentieth centuries, after World War II their experiences began to converge. For this idea to be more than intelligent wishful thinking, it too will need more research.

The chief problem with this book is that one looks in vain for any initial explanation of why such a study is worthwhile. How can a historical study of social mobility contribute to an understanding of social change during the industrialization process? Does a knowledge of historical trends relating to educational opportunity contribute anything to our perceptions of contemporary social-educational problems? Occasionally Kaelble makes his purpose explicit, as when he connects the problem of social mobility with the difficulties associated with German modernization. Alternately, an implicit purpose can be divined when he refers to research done on the recruitment of elites or the effect of political reforms

on equal opportunity. In addition to Kaelble's extensive references and clear, expository prose, the presence of a strong initial justification for this collection of essays would make them seem less sterile.

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CARL H. PEGG. *Evolution of the European Idea, 1914–1932*. Chapel Hill: University of North Carolina Press. 1983. Pp. x, 228. \$28.00.

Carl H. Pegg has presented a virtually encyclopedic account of all the various manifestations of the "European idea" from the beginning of the First World War to the coming to power of Hitler. His broad definition of the "European idea," by which he means any expression of the desire to form a union of the European nation-states, permits him to range across the writings and speeches of politicians, business and trade union leaders, intellectuals, and the amateur manipulators of public opinion in whom the interwar years abound. Nothing escapes his attention, whether it is an impassioned debate on the benches of the Collège Libre des Sciences Sociales in Paris or an obscure reference in some Norwegian newspaper. His goal is not to investigate the evolution of ideas on European unity of individuals but rather to give in a vast mosaic the innumerable forms in which the idea is presented in the words of individuals widely separated in space, national culture, and political affiliation. In short, he wants to show that the idea of Europe has developed a life of its own and that the nature of that idea varied in the two decades he is studying as a result of events of Continent-wide importance—the First World War, the French occupation of the Ruhr, the spirit of Locarno, the Great Depression, and so on. Although one worries occasionally that this compilation (which is the result of many years of reading by the author in a vast number of multinational sources) lacks nuances, in general Pegg is successful in making the European idea speak for itself.

He identifies, quite helpfully for those who wish to understand the evolution of the European movement after 1945, three basic themes that occur again and again in the writings and speeches of this period: the desire to link the movement to unite Europe to the broader peace movement both at the European and at the world level; the need to find some solution to the problem of Franco-German antagonism; and the desire to benefit economically from the integration of the national economies. His assertion that the central ideas of the 1920s re-emerge after the Second World War is immediately verified when one reads the famous press conference of May 9, 1950, when Robert Schuman launched the European Coal and Steel Community.

Pegg does have his favorites, however. The most obvious is Count Richard Coudenhove-Kalergi, with his pan-Europa movement, an integration movement of undoubted good will but little practical accomplishment. More interesting and original is his investigation of the manner in which politicians from Austrian Chancellor Seipel to French Premier Herriot flirted with the notion of a European federation and absorbed the vocabulary of the European idea until it became almost a commonplace—and hence, of course, less effective as a call to action—in their pronouncements on foreign policy.

The format of the book is the cause of its weaknesses. Study of a few individuals, such as Aristide Briand, could have been combined with the broader coverage of the idea. Far too little space is given to business or labor leaders, even in relation to the disputes over formation of a customs union. Of particular importance is his unwillingness to estimate the importance of anti-Soviet, and more specifically anticommunist, feeling in stimulating a move to Europeanism as a form of self-protection.

Pegg's book, however, is a very useful overview of one very significant trend in Continent-wide thinking in the interwar years and is a fine complement to volume 1 of Walter Lipgen's study of the European movement after 1945, *Die Anfänge des europäischen Einigungspolitik, 1945–1950* (1977). One closes the book with sadness for lost opportunities. What fine ideals these politicians and intellectuals had! How could they have achieved so little? How could they have allowed Hitler, as Napoleon before him, to become the self-proclaimed uniter of Europe?

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DAVID FELIX. *Marx as Politician*. Carbondale: Southern Illinois University Press. 1983. Pp. xi, 308. \$27.50.

This curious work revolves around David Felix's definition of "politician." Felix contends that although Karl Marx was "one of the greatest thinkers in world history," he was "a greater politician" (p. ix). This focus on a kind of politics differentiates Felix's book from ordinary Marx biographies. Felix develops his argument while covering the familiar ground of Marx's life, stressing literary, scholarly, and journalistic activities quite as fully as overtly political involvement in conspiracy and agitation.

Most of Marx is subject to debate, and Felix does not dodge controversy. Sometimes, indeed, he strives for outrageousness, as in declaring National Socialist "an accurate name for the party Marx would like to have led in Germany in 1848–49,

nationalistic, socialistic, and as anti-Semitic as tactically useful" (p. 217). But while Felix contends with many Marx scholars, he unaccountably fails to confront Richard N. Hunt's *Political Ideas of Marx and Engels*, ignoring that major work.

On one level, Felix emphasizes Marx's disagreeable traits, showing him as uncomradely, bigoted, dishonest, bullying, and spiteful, in the manner of Leopold Schwarzschild's classic hostile biography. But throughout this account of a Marx so odious as to be expert in alienation—of himself from all potential comrades save for wife, children, and Friedrich Engels—recur Felix's assurances that Marx was almost invariably *successful*, showing "impeccable revolutionary timing" (p. 160), ever the masterful "politician."

Obviously, Felix's concentration on Marx as *politician* does not mean simply that Marx's ideas emerged from political realities. This argument would be defensible although hardly surprising, since few are likely to believe Marx divinely inspired, and it is in any case sound Marxism to root theory in objective conditions. Felix means something quite different: here is the significance of his otherwise bewildering praise of Marx's political brilliance.

For Felix, Marx's ideas do not reflect changing political conditions; rather, Marx's writings are a means for asserting power, and are therefore assessed in terms of alleged political *effect*. Thus, for instance, Felix discusses *Capital* not as an escape from politics, a by-product of Marx's frustrating inability to control contemporary political events, but as itself "an instrument of power," inducing people to act as Marx wished.

Marx controlled and controls others' behavior through his writings and through "The Marxian Revolutionary Machine" (p. 182), composed of people like Engels (despised by Felix as a masochistic writer of "characterless prose" [p. 127], although Felix dislikes Marx for being mean to him); Ferdinand Lassalle, Wilhelm Liebknecht, and August Bebel (variously uncomprehending or uncooperative); and others, long after Marx's death. "As Marx's power increased, his ability to control that power had diminished comparably," Felix explains (p. 189). As Marx's power increased it passed to such as Lenin, who "had known to what degree to violate Marxian theory [while] Mao had known to violate it absolutely" (p. 212). Also instruments of Marx's posthumous power are the Fabians, Keynes, fascists, Felix, and everyone else: "We are all Marxians in a Marxian world" (p. 218). Marx's power has evidently increased (dialectically) to absolute uncontrollability and vagueness, providing a self-contradictory conclusion to a lively book.

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PATRICK BRANTLINGER. *Bread and Circuses: Theories of Mass Culture as Social Decay*. Ithaca: Cornell University Press. 1983. Pp. 307. \$24.50.

It is gratifying, in this age of sociologizing history, to find an occasional worthy contribution to intellectual history that still, in the older manner, unabashedly treats the ideas of published and often important (or elitist, if you will) thinkers, rather than the trends of popular culture alone. Such is the evident goal of Patrick Brantlinger in *Bread and Circuses*. Brantlinger, the editor of *Victorian Studies*, here examines, as his subtitle announces, theories of mass culture as social decay, most notably in the British, German, French, and American nineteenth and twentieth centuries, but with glances at ancient Greek and Roman writers and at the Enlightenment of the eighteenth century. Mass culture, to be sure, is the object of these theories, but Brantlinger's work focuses more often on the theories than on the object itself. His central theses are that a "negative classicism has become the major myth of our time" (p. 42); that the doom of empires and of cultures has often been seen as an extension of mass culture, with its frequent orientation toward "bread and circuses"; that this pessimism, sometimes justified, has nevertheless been exaggerated; and that utter cultural blight is not necessarily prefigured even in today's mass media (notably television), however culturally debased they may often appear.

In pursuing these themes, Brantlinger's approach is alternately insightful and problematical. Certainly an admirably broad scholarship is evident, together with a high level of accuracy. An inexact citation of Denis Diderot and a possibly misplaced emphasis on Gustave Flaubert's "decadence" represent rare and minor exceptions. Other aspects are more troublesome. There is, for example, the bias cheerfully admitted at the outset: that this study has set out to demonstrate a particular thesis—the coincidence and compatibility of mass culture and true democratic community. There is also the frequent, confusing extension of "classicism" to cover the yearning for any elitist past, not just for classical antiquity. There is, finally, the too common jumble of authors and periods, admirable for demonstrating breadth of vision but not always well organized. More satisfactory are the sustained analyses of temporal units and of clearly specified topics, such as themes of decadence or Gothic revivalism in the nineteenth century, or the well-focused encapsulations of such individual authors as Sigmund Freud and Albert Camus. Along the way, moreover, there are substantial insights that are worthy of reflection—insights, for example, on the equivocal position of religion vis-à-vis elitism and mass culture or the hitherto insufficiently noted recurrence of classicist nostalgia in essentially nonclassicist ages. If Brant-

linger's study is far from definitive—and he makes no claim that it is—this must be the common fate of quasi-polemical works on subjects where passions run high. The book remains useful (there is, incidentally, a good index but no formal bibliography) and thought-provoking. Perhaps one should not ask for more.

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STANLEY COHEN and ANDREW SCULL, editors. *Social Control and the State*. New York: St. Martin's. 1983. Pp. vii, 341. \$27.50.

Social Control and the State is a collection of thirteen essays by sociologists and historians. In their introduction, Stanley Cohen and Andrew Scull, both sociologists, emphasize the value of an interdisciplinary approach for analyzing actions by individual groups or the state that might be described as "social control" efforts. They also maintain "a quantum leap" has occurred in the quality and quantity of work produced in roughly the last decade by those studying such questions (p. 1). The essays, seven of which are reprinted items, are divided into two sections. Part 1 presents "reviews and debates" on three general topics: the idea of social control, crime and punishment, and psychiatry and incarceration. The seven essays in this section focus primarily on France, Britain, or the United States in the nineteenth and twentieth centuries. Three major themes emerge. The authors stress that the term *social control* should be avoided unless set within a precise sociological framework. A second basic theme is that the "humanitarianism" of "reformers" must be taken seriously but must also be subjected to close, hard-nosed scrutiny. Finally, the essayists often note that the scholarly debates over what were or were not attempts at social control have been downright vicious. Although this point is not emphasized, such acrimony is, of course, not mere infighting among "ivory tower types." The issues raised in questioning the potentially selfish motives of governments, classes, or groups carry clear implications for modern-day policies.

Part 2 offers case studies designed to illuminate whether and how governments or groups have pursued social-control objectives. Intriguingly, several essayists in part 2 use the term social control without placing it in the kind of framework called for in the essays in part 1. Paul Rock, in a wide-ranging essay on law, order, and power in England, holds that, as late as the early 1700s, "policing was most irregular" and areas of the nation and segments of society, especially the upper classes, "were virtually autonomous of formal social control" (p.

203). Paul Haagen maintains that debt law in eighteenth-century England served as a "means of influence and control" and was "well suited to the needs of England's ruling classes" (pp. 233, 229). Robert Castel utilizes nineteenth-century French sources to offer historical background for analyzing the development of psychiatry. He maintains that the general support for "moral treatment" was rooted more in a concern for protecting the dominant social values, including respect for hierarchies, than in scientific theories. Julie Brown effectively shows that in tsarist Russia sincere efforts to expand and improve treatment for the insane foundered when the financial costs of reform proved more than governments were willing to bear. Steven Spitzer, in a rather polemical style, draws on a variety of geographic areas to argue that in precapitalistic societies "structures of domination were dependent on *extensive*, *indirect* and *ceremonial* forms of coercive regulation" and that under capitalism such "controls" became "far more *intensive*, *direct* and *banal*" (p. 315). All of these case studies have merit, particularly for specialists. But the prize essay of part 2 is Nicole Rafter's examination of the women's reformatory at Albion, New York, from 1894 to 1931. Rafter amply demonstrates that Albion performed a social-control function based on the ideals of the middle and upper classes. Albion aimed to curb the freedom, especially the sexual freedom, of young women and to train them to be good domestics and mothers. Freedom of choice in either future life style or work opportunities was decidedly not part of the Albion plan. Yet Rafter also shows that the women reformers who championed Albion were motivated, in part, by humanitarian concerns; moreover, at least some Albion inmates later came to view their experience as a key to happiness. Thus, we get a judicious interpretation that reveals the complexities of motivation and action. This essay can usefully be assigned to students as a model study of social control.

Considering the volume as a whole, there is an unevenness in coverage of major exponents of a social-control thesis. The work of Michel Foucault receives extended analysis in a number of essays. David Rothman, who contributed a short, thoughtful item to the volume, also receives a good deal of attention. Yet the significant work of Frances Piven and Richard Cloward never receives extended analysis. This omission is surprising and unfortunate. Specialists, even those who follow Rothman's wise advice to note "all the grey in the texts" (p. 106), will reasonably question points made in several of the essays. And not all will agree with the volume's general support of the view that states and groups within states have often pursued policies to keep socioeconomic power in the hands of those who already possess it. Nevertheless, the work offers something for everyone interested in understanding

and exploring what is commonly called social control.

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XAVIER DE MONTCLOS. *Les chrétiens face au nazisme et au stalinisme: L'épreuve totalitaire, 1939-1945*. Paris: Plon. 1983. Pp. 303. 75 fr.

In the eastern areas occupied by the Germans during the Second World War there was a great religious revival. Not that the Germans had anything directly to do with it, nor would the Nazis have tolerated it in the long run. But it showed that religion had not been stamped out by years of Soviet oppression; far from it. Religion was to play some part in the outcome of the war. Stalin met the patriotic willingness of Russian Orthodoxy to defeat the German enemy with an address to "Comrades! Citizens! Brothers and Sisters!" (the latter a Christian greeting in the Russian Orthodox Church [p. 65]), restored the patriarchate, and drew on the resources of the faithful. Later the Soviet regime was to get back to the business of persecution as usual.

Unfortunate were those caught in the middle. Hannah Arendt once wrote (with the purges in mind) that totalitarian rule can present the individual with choices that are not between good and evil. "When a man is faced with the alternative of betraying and thus murdering his friends or of sending his wife and children for whom he is in every sense responsible, to their death; when even suicide would mean the immediate murder of his own family—how is he to decide? The alternative is no longer between good and evil, but between murder and murder." The Lithuanian resistance fighters, who fought first against the Soviets, then the Germans, and then the Soviets again, would have understood. After becoming acquainted with the methods of the NKVD, they arranged, when escape was impossible, to die in ways that would make them unrecognizable, so that their families and friends would not be deported. Institutions also faced such choices. When the Roman Catholic Church in the Netherlands protested the treatment of the Jews, in reprisal the Germans rounded up and deported the baptized (Catholic) Jews as well. Protestant-baptized Jews were for the moment left alone.

In Croatia there was a different sort of situation in which to get caught. There murders of the Serbian Orthodox were perpetrated on a mass scale by the *Ustaše*, who created what the author calls a "Catholic totalitarianism." The mutual bloodletting in Yugoslavia was among the more horrible features of the Second World War. There seems to have been little conflict of conscience there, only bloodlust of a

primitive sort, in which religion played the role of defining the enemy.

Xavier de Montclos finds the record of the churches and of the papacy a mixed one in the face of the totalitarian test and places his future hopes in the individual conscience, which he thinks should have been encouraged and freed. Cardinal Tisserant urged that alternative to Pope Pius XII during the winter of 1939–40, and the author wishes that there had been an encyclical on the duty of the individual to follow his or her conscience. For “when the totalitarian test is upon us,” he writes, “it is the incorrigibility of the spirit which gives societies the ability to survive and to rise again” (p. 279). Perhaps so. In any case, encouraging consciences in disobedience to evil government could at least preserve the capacity, if not always the opportunity, to choose between good and evil.

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MICHEL RAGON. *The Space of Death: A Study of Funerary Architecture, Decoration, and Urbanism*. Translated by ALAN SHERIDAN. Charlottesville: University Press of Virginia. 1983. Pp. 328. \$20.00.

MICHEL VOVELLE. *La mort et l'Occident de 1300 à nos jours*. (Bibliothèque Illustrée des Histoires.) Paris: Gallimard. 1983. Pp. 793. 300 fr.

Michel Vovelle's introduction suggests two “models”—to use a term he favors—with which to compare his vast new book. The first, a view of “human history in the mirror of death” (p. 7), is a synthesis of evidence on practices and attitudes surrounding human mortality, designed to serve as an introduction to modern Western civilization. The second is an analysis: the reflection in the mirror is “singularly distorted” (p. 8) and must be deciphered. To be understood, changing perspectives on death should be related to fluctuations in the “happiness expectancy” (p. 25), a measure less crude, though more complex, than life expectancy. Vovelle aims not only to trace the view of death through the ages but also to contribute to the sociology of cultural change by analyses of social and demographic variables associated with the imagery of death.

Undertaking the first task Vovelle describes pagan and Christian views at the start of the fourteenth century, chiefly as their contrast was reflected in literature and in funerary art. The apparent change in beliefs through the course of the next two centuries represents the struggle of the church to separate the dead from the imaginary world of the living, to organize conceptions of the world beyond, and to spread more widely and deeply the idea of an

individualized Last Judgment. Catholicism succeeded, by and large, in turning the ancient ghosts into demons, reducing communal and family control over dying and dead members, and introducing the priesthood into rituals associated with death.

As the “triumph of Death” over “the dead” is traced in the second part of the work, the range of types of evidence broadens to encompass cathedral sculpture, cemetery architecture, and books of the hours. To Vovelle, they show the rise of panic at the idea of death in the years after the Black Death and, in the first half of the fifteenth century, such signs of increased “investment in the hereafter” as testamentary prescriptions refining burial procedures and multiplying means for commemorating the deceased.

For Vovelle, the Renaissance of the sixteenth century saw an integration of views of death with everyday family life. To the sterile ratiocinations of Lucien Febvre on the impossibility of nonbelief in the world of Gargantua and Pantagruel, *La mort et l'Occident* opposes the findings of Mikhail Bakhtin on the resonance of François Rabelais's work with a humane popular culture. Although Vovelle sees this humanism as an underlying trend both for Catholicism and the Reformation, he discloses radical differences between the two persuasions in the practices with which they met death. In neighboring settlements of Provence after 1560, for example, Protestants concentrated on charitable works and this-worldly evidence of piety in their testaments, while Catholics persisted in earlier concerns with the seemliness of sepulture, funeral, and altar observances. In both, multiplication of the *livre de raison* reflected a heightened sentiment of family continuity in the presence of death.

From the sixteenth-century celebration of life, Vovelle turns to the “tragic” atmosphere of the seventeenth century. An elite mysticism that made death the main concern of life eventually bore fruit in the spread of devotion at lower social levels. Yet the asceticism of the pioneers of counter reform was transformed as more numerous classes of society found renewal of faith in depictions of Mary the Madonna rather than in Pietà and other images of the martyred Christ.

The more variegated artistic and literary visions of death from which Vovelle selects examples to illustrate the progress of de-Christianization from the baroque age through the nineteenth century are presented against a background that shows a simultaneous trend for Christian versions, chiefly based on fear, to penetrate more widely among the urban and rural poor. To concluding chapters on post-World War trends in mortality and their cultural counterparts—ranging from anguish and horror in German expressionism to denial and taboo in Hollywood—Vovelle adds a chapter on the “rediscovery

of death," to which he has here perhaps made one of the most ambitious contributions.

As a survey of the history of Western culture how should *La mort et l'Occident* be rated? For variety of evidence with a potential for attracting the talented but perhaps dyslexic student with a Bach cantata, an illustration from *Justine*, a *Star Reach* cover, or a photo of that world-famous, but hardly familiar, American drive-in mortuary, perhaps highly. For the Continental European student the sources drawn on are commendably cosmopolitan: Vovelle is at home among Anglo-Saxon artifacts in the New World and in the Old and apparently is comfortable in Slavic and Scandinavian Europe as well as in the cultures of Italy, France, and Spain.

But how to use these riches? There is no index. The bibliography comprises only a portion of references to secondary works, sometimes sketchily cited in the text, and fewer still of the primary sources used. Many of the illustrations lack dates and locations. There is no list of tables and graphs. These, indeed, are often "thrown away": a set of percentages may be embedded in the text or, if legibly set off from it, require the reader to provide his own title and, sometimes, plausible names for the components of the series depicted.

And how is it for analysis? Vovelle's fruitful searches among New England epitaphs and Provençal testaments are well represented here, but without the careful explanation of method that allowed readers to weigh conclusions in his articles. The "laboratory," to paraphrase his quotation from Marc Bloch, sometimes seems in disarray. For a nonrandom, but miniscule, sample: what is the universe from which a sample of paintings is drawn or a census taken (pp. 308–12)? How are we to understand a change in the qualities celebrated by epitaphs in an Aix cemetery as a reflection of changing values without knowing age or sex distribution (p. 614)? Should we not be given evidence for the assertion that the seventeenth century saw "strengthening of paternal power, in aristocratic elites as well as the rural world" (p. 272)?

As for theoretical underpinnings, Vovelle promises, at the start, to avoid crude extrapolations from the mortality rate: preoccupation with death is not a mere function of life expectancy. But he later refers to the "demographic determinism which we have evoked," finding it has its limits in explaining the dolorous aspect of seventeenth-century France and cannot, as it could in "the decline of the middle ages, explain everything (*rendre compte de tout*)" (p. 314). And, although at the end of the eighteenth century he finds that social crises, "felt more directly in population flux, made it unnecessary to search farther," henceforward he feels that the relationship between collective representations of death and social well-being will become increasingly complex

(p. 475). More complex, indeed, do they become as he attacks the question of the effects of industrial capitalism on the quality of life and life expectancy in nineteenth-century cities—and after. Truly, "*tout est lié*" (p. 7). The admirable thing is that, knowing this at the beginning, the attempt to find the links has been made at all.

In Michel Ragon's essay, an architect, decorative artist, and student of urbanism takes the reader on a pleasant ramble that ranges from the pyramids of ancient Memphis to a vision of the unity of the universe in which the "space of death" will be diffused in the "urbanism of the cosmos." Anecdotal rather than chronological, the work lingers longest in the Paris of the eighteenth and nineteenth centuries, where it will be discovered that the "fashion for pyramids in French art existed . . . long before Napoleon" (p. 241)—a fact that may help to explain the apparent success of current plans to install a pyramid in front of the Louvre. Baron Haussmann has a short chapter to himself, followed soon after by the inevitable "American Way of Death," where, however good-naturedly, Evelyn Waugh's *The Loved One* seems to be mistaken for a historically reliable document (p. 293).

ELIZABETH WIRTH MARVICK
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ROSS EVANS PAULSON. *Language, Science, and Action: Korzybski's General Semantics—A Study in Comparative Intellectual History*. (Contributions in Intercultural and Comparative Studies, number 9.) Westport, Conn.: Greenwood. 1983. Pp. viii, 163. \$29.95.

Count Alfred Korzybski (1879–1950) is remembered as the founder of the general semantics movement, which was popularized in the United States as a reforming social program. Korzybski himself saw his work as a broad and scientific attack on conventional (Aristotelian) philosophy of language, based on a critique of logic and promising, among other things, a therapy for the mentally disturbed and a path to world understanding. Ross Evans Paulson calls this work "the story of the origins and fate of Korzybski's general semantics [which] thus provides a microcosm of the general problem of the transfer of ideas from one culture to another and their acculturation" (p. 7).

Unfortunately, the work seems a parody of contextualist intellectual history, with its backgrounds, influences, currents of thought, cultural conditions, trends, and so forth. For Paulson any text is to be seen as the product of environments—intellectual, social, political, what-have-you—from which it will absorb, reject, mix, misunderstand, adapt, or transform ideas that can always be attributed to some preexisting text. Consequently, it is environments

that get the most attention, in the form of brief paraphrases of the ideas of a large number of figures; in this parade, a paragraph is generous (for example, the treatments of Jan Łukasiewicz, Stanisław Lesniewski, Tadeusz Kotarbiński, Leon Chwistek, Ludwik Krzywicki, Eliza Orzeszkowa, and many more); at times a sentence or two suffices (Tadeusz Ródoziewicz, Jerzy Grotowski, Miron Białoszewski, Zbigniew Bieńkowski, and Karol Wojtyła [later pope]). In addition, Paulson is fond of the use of "types" (the engineer or the free intellectual) and "themes" (engineering millennialism or the revolt against positivism); Korzybski's first book, *Manhood of Humanity*, is thus "a typical example of the millennialist books that appeared following the catastrophe of World War I" (p. 27).

Even if one accepts the assumptions of a contextualist intellectual history that traces ideas to environments and sources, Korzybski, as presented here, is a poor candidate for absorbing ideas. He is shown as a one-way communicator (p. 71), whose "ideas had long since hardened" by 1933 (p. 63). He always seemed to be misunderstood. Despite his passion to improve mankind by purifying language in the interest of understanding, Korzybski's followers were very confused about his goals. Paulson suggests that even Korzybski's wife had misrepresented his ideas during his lifetime (pp. 66–67). Clearly, practical semantics were not Korzybski's forte. Paulson suggests from time to time that general semantics is the product of an individual, idiosyncratic mind, but rather than attempt to find out what that mind was like, he draws us back to some context. He would rather not read Korzybski, explicate and illustrate his ideas, weigh and analyze his language, or tease out the forces and voices contending in his texts. The brief and extremely abstract paraphrases offer little sense of Korzybski or of his appeal.

This is a work of "comparative intellectual history," contrasting the Polish intellectual milieu with the American, insofar as Paulson will grant the United States any intellectual life at all. The Polish situation is presented as a series of brilliant men and women, philosophers and poets, whose ideas formed the raw materials for Korzybski's work, but the American scene is all a social history of reception—boobocracy in the 1920s, depression in the 1930s, fanatical and shallow seekers of social redemption, "positive thinkers" in the 1950s, and mere popularizers (like Stuart Chase) or mildly liberal academics (like S. I. Hayakawa). At one point Paulson asks, "Where in an America that has institutionalized its academics, trivialized its education, debased its public discourse, and denied its free intellectuals the dignity of secure status—where in such a culture is one to look for the word that will not only hurt but heal, not only command but bring

communion, not only deceive the mass audience but decipher the mysteries of human destiny?" (pp. 84–85). Where, indeed? Perhaps in postwar Poland?

Poor Korzybski is lost and almost beside the point here. What is unique about his work, its status as a creation, is not suggested. Instead, we read that his philosophical ideas were being "undermined" by the course of European thought at the very time that he was finishing his system. If he had but kept in touch with his European roots, he might, Paulson suggests, have seen that his foundations were "eroding away" (p. 63). It is not made clear whether this would have led Korzybski to get things right or to lapse instead into mystical silence. What might have been a useful presentation of Korzybski's work and its contribution to a clearly defined problem at a certain time (like Jonathan Culler's *Ferdinand de Saussure*) has been overwhelmed by its apparatus and assumptions.

HANS KELLNER

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ALLEN GUTTMANN. *The Games Must Go On: Avery Brundage and the Olympic Movement*. New York: Columbia University Press. 1984. Pp. xiv, 317. \$24.95.

For almost fifty years Avery Brundage was the symbol of international sports and the driving force behind the transformation of the Olympics from an antiquarian, aristocratic pastime into the second most popular international sports competition. He was an athlete, "self-made" millionaire, leading figure in the pro-German isolationist movement in the 1930s, and a staunch crusader for his version of amateurism. Allen Guttman's works on American conservatism and the development of the modern sporting traditions make him uniquely qualified to write about both Brundage and the Olympics. Guttman has done prodigious research in the papers of Brundage, his colleagues, and major sports organizations. Any future study of the Olympics or Brundage must start with this book.

The Games Must Go On is true to its title and subtitle; it is not a biography in the traditional sense. There is little effort to explain Brundage from a view other than his or that of a convinced Olympian. Guttman does not pose the question, "at what price?" in apposition to his title. Brundage's devotion to an ideal is a central theme, but that ideal is not defined until almost halfway through the book. It is difficult to come to terms with Guttman's view of Brundage unless we accept that Brundage's flaws were either caused by his overly zealous dedication to the ideal or were merely gaffes in public relations. If, for instance, we accept the view that Brundage refused to admit the paternity of his sons because of his fanatic attachment to the religion of olympism,

how can we view his other actions? Brundage's anti-Semitism is explained as a reaction to the outcry against the 1936 Berlin Olympics, his ruthless dealings with rivals within the Olympic movement are described as efforts to protect the purity of the cause, and his insensitive and offensive effort to equate the murder of Israeli athletes in 1972 with the exclusion of Rhodesia from the games is dismissed as "his tactless self" (p. 254). Unquestionably the book will raise passions as well as alternative explanations.

Brundage distrusted the principles of those men who disagreed with him and cast a blind eye at events that did not fit with his prescribed view of the world. A reasonable assessment of the opposition to Brundage is that it was based not so much on what he said as on a consistent pattern of what he did. Guttman's first paragraph includes the comment, "Whether we think of him as idealistically inspired or as quixotically unrealistic, he was one with his ideals" (p. 261). These choices unnecessarily require the reader to accept that Brundage's actions and words were not motivated by either malice or by his dedication to another ideal: the importance to him of retaining and exercising power and using international sports to sanctify a political and social order. Brundage understood the influence of sports and did not shirk from using it while castigating others for attempting to further their own causes.

However the reader interprets Brundage after reading this book, there is no doubt that Guttman has shown why Brundage was important and that historians cannot ignore the role sports has played in shaping the values of contemporary society.

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ANCIENT

HERBERT SCHUTZ. *The Prehistory of Germanic Europe*. New Haven: Yale University Press. 1983. Pp. viii, 421. \$45.00.

Here is a book that should be read by anyone the least bit interested in the history of Europe. The title says "prehistory" but that period of time, stretching from the first appearance to the earliest written record of the peoples who lived there, is all part of the full history of that part of the world. Prehistory is really history in depth and this book is an admirable excursion into that vital area.

Until only very recent times, most European peoples lived in small cultural and geographical units—families, clans, tribes, and villages. Anthro-

pologists and archaeologists call these units "cultures" and they have very long lives through centuries and millennia of history. Anyone wanting to understand the depth of European traditions should know something of these earliest roots. That is what the body of this book concerns. Herbert Schutz, writing about the great Neolithic Age culture complex in the Balkans now called "Old Europe," puts it succinctly: "The 'cradle' of European civilization is located instead [of in the Mediterranean basin or Southwest Asia] in the plains of the Danube and the river valleys of its tributaries" (p. 58). That tradition lasted intact for more than three thousand years and its effects are on Europe still. Add to that the Indo-European speakers probably hailing from the eastern and northern periphery of Old Europe, and many of the continent's cultural foundations become clearer. When will the publishers of Western civilization textbooks and all teachers of that important subject realize this elemental fact of European history? Schutz's book stands as a corrective.

The author is, in truth, more modest. The book begins with a survey of the cultures of the Old Stone Age, passes on to the colonization of Europe by the first farmers in the New Stone Age (sixth millennium B.C.) and then discusses the integration of cultures in the Bronze Age. These early chapters might bewilder the reader who is unfamiliar with the many names archaeologists have given the cultures of those times. Nevertheless, Schutz strives for clarity in these difficult matters and largely succeeds.

The book picks up pace, and likely interest for the general reader, with the first Iron Age (Hallstatt), the Celtic epiphany of La Tène, and the early Germanic peoples in the last chapter. Drawing on material already familiar through popular archaeological literature such as the "bog people" and the glories of Celtic art, the volume is at its best here describing religion, social order, economy, and everyday life.

If there are criticisms of the book they lie in the very nature of a work such as this. Specialists in the field of European prehistory will find much of the surveyed material familiar (though the author does make judicious comments on its meaning throughout) while novices may struggle with the earlier chapters. Still, this is a very useful survey.

Most of the illustrations are good, the color plates are very nice. Maps are rather crude, though legible. Also note that the bibliography is a bit dated, mainly running up to the mid-1970s. This excludes such works as R. J. Harrison's book on Beaker Folk and Patricia Phillip's survey of European prehistory among other recent publications. The author might have also consulted the *Journal of Indo-European Studies* for cogent material. But these omissions are minor in view of the good survey of German

literature, particularly of the work of the eminent Müller-Karpe.

BRUCE KRAIG
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CHARLES WILLIAM FORNARA. *The Nature of History in Ancient Greece and Rome*. (Eidos, Studies in Classical Kinds.) Berkeley and Los Angeles: University of California Press. 1983. Pp. xiv, 215. \$22.00.

This is a learned, vigorous, and valuable account of the theory and practice of historical writing in the Greco-Roman world. It is not a series of essays on individual historians, but an integrated analytic study of the Greek and Roman historical authors organized topically and treated comparatively. The chapter titles reveal the main topics examined: "History and Related Genres," "Research, Orientation, and Explanation in the Greek and Latin Historians," "The Theoretical Foundations of Greco-Roman Historiography and Their Application," "The Speech in Greek and Roman Historiography," "Points of Contact between Historiography and Other Genres and Modes of Thought," "Modern Historiography: An Epilogue."

There is space here to take note only of a few contributions of this rich and thoughtful work. Charles William Fornara pays homage to Felix Jacoby, whose work provides the basis of our study of ancient historiography, but he rightly demolishes Jacoby's rigid definition of history to exclude all ancient historians from it as mere writers of *contemporary* history. Fornara provides a neat and accurate definition of history as practiced by the ancients: "the description of *res gestae*, man's actions in politics, diplomacy and war, in the far and near past" (p. 3). He likewise rejects Jacoby's rigid and untenable theory of the development of ancient historical writing in which Herodotus's history is the successor of the genealogist-ethnographer-geographer Hecataeus. Instead he persuasively argues that the more appropriate model is Homer: "Herodotus' Histories represent a fusion of prose and poetry, of the Homeric epic and a now transformed Ionian historiography. History thereafter remained a mimetic genre, devoted to the description of the memorable actions of men" (p. 32).

Fornara is enlightening and persuasive in showing how Roman historiography, while powerfully influenced by Greek models, and in fact descending from them, was different in fundamental ways. Greek historians, though they may have played a part in events, were primarily observers, researchers, and writers, while their Roman counterparts were "active members of the ruling class" whose claim to attention and belief came less from their investigation of the facts (*historia*) than from the

authority adhering to them from their experience as statesmen and generals (*auctoritas*). The Greeks aimed at breadth, "the whole known world or its substantial portions" (p. 53); the Romans focused on their own history, their wars and internal struggles. The Greeks' interests were various while the Romans increasingly concentrated on internal politics. This led to analyses of Roman history that moved from "optimism to grim diagnosis to retrospection." The style of historical explanation moved away from the Greek concern with "pragmatic causes" to "the cultural philosophical question of Rome's decline" (p. 58).

In general Fornara defends ancient historiography, denying that it was inadequately concerned with truth and accuracy and unduly influenced by rhetoric. He makes a persuasive case that ancient writers understood that the historical genre required adherence to the truth and that, in spite of inevitable lapses, their practice was consistent with their theory. His excellent discussion of the general reliability of speeches in ancient histories includes a splendid defense of Thucydides' Melian Dialogue. This is an intelligent, provocative, and important book that deserves serious attention.

DONALD KAGAN
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INGEBORG SCHEIBLER. *Griechische Töpferkunst: Herstellung, Handel und Gebrauch der Antiken Tongefäße*. (Becksche Archäologische Bibliothek.) Munich: C. H. Beck. 1983. Pp. 220. DM 38.

This relatively small volume is part of a German series devoted to specific aspects of the Greek and Roman world. The subject is Greek pottery, but it is approached from a slightly different direction than usual. Pottery shapes, painting styles, and history are treated only in passing, the author, Ingeborg Scheibler, being more interested in the pottery industry itself and the uses of that industry's products in the framework of Greek civilization in general.

The work is divided into three major parts with an introduction and a conclusion. The first portion outlines the general characteristics of Greek pottery, including evidence for use as well as abbreviated comments concerning shapes and decoration. The central part mainly deals with technical matters of manufacture, including the difficult topic of workshop organization. The last section is concerned with the centers of manufacture of Greek vases and their markets, both local and foreign. The text is liberally sprinkled with clear and appropriately chosen illustrations, which are well integrated into the discussion. Although chosen from a variety of sources, the illustrations are well done and some

new and interesting subjects are shown. Each of the major sections of the book has its own short bibliography at the beginning of the notes for that part. There is no index.

There is really nothing like this book currently available. In a sense it demonstrates one of the new directions that the study of Greek pottery has taken in recent years with an emphasis on an attempt to understand the use and meaning of the Greek vase in ancient society. This is a particularly difficult thing to do, mainly because of the paucity of evidence together with the ambiguity of what evidence does exist. This book is not an exhaustive study of one aspect of the subject but rather a brief overall view, essentially a handbook. This form does not allow the author to delve deeply into particular problems and she occasionally manages simply to skirt a complex issue or controversy. Generalizations are of course a necessity, and some will probably excite the knowledgeable reader. Although the documentation is generally quite full and complete, a few obvious gaps are noticeable as well as a number of unfortunate typos, which cannot be listed here for reasons of space. The reader might wish that some footnotes were a little more informative, especially in the case of controversies, and there are a few startling omissions, such as Joan Merten's important *Attic White Ground* (1977) and Beth Cohen's *Attic Bilingual Vases and Their Painters* (1978). I find the lack of an index to be a serious omission, even in a work of modest size, given the large amount of information between its covers.

This would be a useful book to have for teaching and one hopes that an English version might soon appear. It would, however, have to be used with caution. Nevertheless, this work provides a great deal of information on Greek pottery that has not been brought together in one place before, as well as new insights, which will provoke discussion and deserve attention.

WILLIAM R. BIRS
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JOHN V. A. FINE. *The Ancient Greeks: A Critical History*. Cambridge: Belknap Press of Harvard University Press. 1983. Pp. ix, 720. \$35.00.

The veteran scholar John V. A. Fine excellently surveys Greek history from the arrival of the Greeks, about 2000 B.C., to the death of Philip II (336 B.C.). The sixth and fifth centuries predominate: eleven chapters take us through the Peloponnesian War, after which come "The Fourth Century" and "Macedonia and Greece"; thus 23 percent of the space goes to the period after 404, the same proportion as in Raphael Sealey's *A History of the Greek City States* (1976).

Fine writes "for those seriously interested in Greek history, whether undergraduates or graduate students." We commend him for discussing in detail so many problems of Greek history; the Themistocles Decree, for example, gets seven pages (two in Sealey). But only readers with some background in the topic and a taste for problems (probably not freshmen and sophomores) will find the treatment easy to follow. It is notable that, in a book written over many years, the author has been able to keep his bibliography and citations up to date.

The spirit of the narrative is candid and modest. The tone may be too cautious at times. We cannot quarrel with the frequent warnings, "a definitive answer will never be attained," "no certain conclusion can be drawn," but a historian of Fine's standing probably has the duty to push his knowledge and inferences to bolder conclusions. Sometimes this happens. On page 204, he is noncommittal about whether Solon established sortition for archons, but on page 404 it is "more probable" that the lot as a political device is not earlier than Cleisthenes (surely right). On the decree about standards (it is not solely about "coinage," nor do we know that Clearchus proposed it), he seems to accept the Princeton-Oxford orthodoxy about sigma and to date it ca. 449–47, "though some are in violent opposition." On another question, what the Areopagus did under and after Solon, he shows refreshing scepticism in warning that Aristotle (*Ath. Pol.* 8.4) "probably had little other evidence at his disposal beyond the tendentious statements and interpretations of the fourth-century polemicists" (hear, hear).

There are no plates, and the book entirely omits philosophy, art, and literature; indeed, there was no room for them in a book that examines politics and war in such detail. Economics gets a few pages; the chapter on the practices of democracy is unusually good. But in the discussion about Cleisthenes, the traditional statement that an Athenian tribe included one trittys from the city, one from the coast, and one from the inland now needs the additional statement that "enclaves" of demes were evidently separated from their geographic neighbors and placed in far-off trittyes, probably to obtain a balance of population among trittyes and tribes: as suggested by John S. Traill, and fully elaborated by Peter Siewert in *Die Trittyen Attikas und die Heeresreform des Kleisthenes* (1982).

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PATRICE BRUN. *Eisphora-Syntaxis-Stratitika: Recherches sur les finances militaires d'Athènes au IV^e siècle av. J.-C.* (Annales Littéraires de l'Université de Besançon, number 284; Centre de Recherches d'Histoire An-

cienne, number 50.) Paris: Les Belles Lettres. 1983. Pp. iii, 193.

Athens's history in the fourth century is tragic. Having been beaten in 404 B.C. by Sparta and its allies, Athens struggled back to a position of strength, only to fall to Macedonia in 338. During these two generations the use of mercenary troops became common, so that the financial strength of the state became crucially important. This book is a description of Athens's attempts to raise money.

For Patrice Brun, the Athenians' financial comeback began with the organization of the second naval alliance in 378. They realized that they could no longer depend on their allies for large amounts of cash as they had before 404. Hence, they organized themselves (and their metics) whose property was worth, probably, 2,000 drachms or more into one hundred symmories that henceforth paid the war tax called *eisphora*. It was collected in most years between 377 and 366.

At the same time the members of the alliance began to pay *syntaxeis*, a new name for the old tribute. Brun ably argues that the amounts required from each state were based on the former assessment of Aristides. The money, however, was not asked for every year. It was never sent to Athens; it was handed over instead to Athenian generals who came for it. The first such payment amounted to 195 talents; it was collected when the Athenians dispatched fleets simultaneously into the Aegean and western waters in 373 and could not carry the cost alone.

These two sources of revenue allowed financial recovery in the 370s. After that Athens's income dwindled. Individuals held back their *eisphorai*, so that about 366 new methods of collection were imposed on them. The three wealthiest members of each symmory now paid *proeisphorai* in a lump sum and reimbursed themselves from the other members. This created much resentment. Here is a weakness in Brun's argument. She believes (from Dem. 22.44) that between 378–77 and 356–55 war taxes amounted to little more than three hundred talents. Many editors of the Demosthenic corpus think that either a number like *chilios* has dropped out of the text or that *triakosi* is corrupt. If true, as is likely (see Dem. 47.54), Brun's point fails.

In 355 Athens's outlook suddenly turned bleak. It lost a war with its former major allies, and thereafter its income from the remaining small ones dropped to a mere sixty talents. (Brun prefers this figure from Aesch. 2.71 to the forty-five of Dem. 18.234.) It was most unfortunate for Athens to lose so much revenue at the very moment when Macedonia was commencing her bid for the hegemony of Greece. Again the Athenians overhauled their finances, this time organizing the Stratiotic-Theoric fund. Brun

argues they were the same treasury; the name changed according to the state of war or peace. Demosthenes' urging the Athenians to commit more money to the war against Macedonia was ignored because many Athenians knew that their regular finances were incapable of sustaining a long war and because the rich refused voluntary contributions. Poverty contributed to defeat at Chaeronea in 338.

Not everyone will agree with all of Brun's conclusions, because, as she is well aware, our evidence is too imperfect to admit of certain conclusions. Nevertheless, this is a stimulating, well-argued book, a useful corrective to the overly schematic reconstruction of Athenian finance given by Rudi Thomsen in *Eisphora* (1964).

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HYLA A. TROXELL. *The Coinage of the Lycian League*. (Numismatic Notes and Monographs, number 162.) New York: American Numismatic Society. 1982. Pp. xix, 255. \$35.00.

The Lycian League has generally received less attention than other Hellenistic federations, and its coinage was last surveyed over three-quarters of a century ago. Hyla A. Troxell's work fills a genuine need. She begins with an able short survey of the historical background and previous scholarship. The league seems to have been formed in the last decade of the third century B.C., and it maintained at least nominal independence until annexed by Rome in A.D. 43. The coinage casts light on league organization, membership, and political relations, particularly with Rome. Troxell does not seek to produce a history of the league, but no history of the league can be written without abundant reference to her book.

Although earlier efforts did little to differentiate among league issues, Troxell distributes the coinage into five periods, each consisting of related groups of either bronze or silver coins. The periods have chronological significance, but there are considerable overlaps since bronze and silver were sometimes coined concurrently. The earliest coinage is entirely of bronze and dates from ca. 200–167 B.C. Little has survived from this period, and few were probably struck. Period 2 covers silver drachms from ca. 167–81 B.C. Troxell correctly insists that two series of pseudo-Rhodian drachms issued about this time must be attributed to Mylasa rather than Lycia. True Lycian League issues of the period bear the head of Apollo and lyre as types and city initials in addition to the Lycian ethnic. Troxell's discussion of the coins substantially revises our understanding of the membership of the league. Period 3 consists

of bronzes of the Lycian League cities issued from ca. 100 to the mid-30s B.C., overlapping the silver issues of period 2.

The coinage of period 4 consists of silver drachms, hemidrachms, and quarter drachms issued from the mid-40s until after 19/18 B.C. Troxell explains convincingly the metrology of these issues, long a problem, and elucidates two contemporary issues of Brutus. Interesting drachms bearing the head of Augustus indicate how nominal was the independence of the Lycian League. Period 5 contains bronzes ranging from the late 30s B.C. to ca. A.D. 40, mainly bearing the names of the two Lycian districts, but occasionally also the names of particular cities. A chapter deals with the location of mints, as distinct from the minting authorities, and appendixes treat misattributions, forgeries, and Claudius's Lycian coinage.

The catalogue sections must be used with care; presentation is not uniform, due in large part to variations between silver and bronze in preservation and survival rates. The reasons for other differences are not apparent.

The plates are good, but the photographs could have been chosen more efficiently. Many obverse dies are illustrated several times, while many reverse dies do not appear at all. Photographs of some coins included in the plates can be found elsewhere with ease, while other coins not photographed, particularly pieces in the unnamed private collections, have never been illustrated.

Troxell's work is a very specialized but worthwhile contribution to ancient studies.

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ANDREW WALLACE-HADRILL. *Suetonius: The Scholar and His Caesars*. New Haven: Yale University Press. 1984. Pp. viii, 216. \$22.50.

Suetonius has long been neglected by English-speaking historians of Rome. Tacitus, a man of superior intellect and greater literary talent, dominates Suetonius's era, and Tacitus's historical writings constitute the most important source of information for much of the period with which Suetonius is concerned in the *Caesares*. By comparison, Suetonius has always seemed to convey trivial, even distasteful material, and so has long been deemed unworthy of serious study. At once, therefore, Andrew Wallace-Hadrill's book fills a gap, and in so doing makes better sense of Suetonius and his writings than has ever been the case before. But the work does much more, for it is an important contribution to Roman social and intellectual history, evoking in compelling fashion the cultural ambience of the Hadrianic age.

The book continues a now well-established genre of studies, inaugurated by R. Syme's *Tacitus* (1958), in which authors of works conventionally used as quarries of source material are examined as products of their own contemporary contexts and their writings then evaluated from that basis. Accordingly Wallace-Hadrill first shows how the scholarly interests of Suetonius led naturally to his emergence as a court official (because of the close connection in his day between literary education and the emperor's service), and he then suggests that the interests visible in the *De viris illustribus* and other works now lost continued directly into, and governed the shape and tone of, the *Caesares*. Suetonius did not set out to write history, but to provide information, often of an antiquarian sort, on Roman life and manners; and as an expert on literary matters he found himself in the employ of emperors who needed such expertise, not least as a basis of advice for the promotion of literary culture expected of them.

Wallace-Hadrill discerns in the *Caesares* a clear method of presentation. The extent to which emperors won their subjects' approval through the maintenance of order and security and the preservation of traditional social norms is a cardinal element of evaluation; while the ethical dimension attendant on the widespread deployment of virtue and vice terminology is determined not by a Plutarchean interest in character, but by the manner in which emperors treated their subjects. The concern with such aspects of private imperial behavior as sexual and religious tastes, while certainly antiquarian, is conditioned, more importantly, by the increasing hellenization of upper-class Roman society. In sum, Suetonius measures his emperors against an imperial ideal and, Wallace-Hadrill argues, captures in the process the essence of the Hadrianic era, with its devotion to systematic learning and the finer aspects of Hellenistic culture.

Inevitably the book raises issues that will require further debate. For example, if, as Wallace-Hadrill maintains, Suetonius had a special interest in the age of Cicero and Augustus, a clear explanation is needed of why he chose to compose biographies of all the emperors up to Domitian. Moreover, comparison of the book with the contemporaneous *Suetonius* by Barry Baldwin (1983) will be essential. But without doubt Wallace-Hadrill has taken the study of Suetonius to a new level and the achievement should be duly recognized. As might have been expected, Tacitus turns out in many ways to be less representative of his age than Suetonius, the scholarly courtier.

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LEONHARD SCHUMACHER. *Servus Index: Sklavenverhör und Sklavenanzeige im republikanischen und kaiserzeit-*

lichen Rom. (Forschungen zur Antiken Sklaverei, number 15.) Wiesbaden: Franz Steiner. 1982. Pp. 253.

In October 63 B.C., as details of Catiline's attempted coup began to emerge, the Roman Senate broadcast an appeal: to any free man who came forward with information, a reward of 200,000 sesterces (a modest fortune) would be given, and to any slave, half this sum plus his freedom. Although the offer was apparently not taken up, that portion of it affecting slaves raises an interesting question: Under what circumstances was the Roman state willing to accept, solicit, or even compel the testimony of slaves against free persons, and especially against their own masters?

The answer involves a balance between competing public interests. On the one hand, slaves were private property, subject by law and tradition to their master's virtually unrestrained dominion; hence comes the maxim that Cicero first constructed, that slaves cannot bear witness concerning their masters. But, on the other hand, in exceptional circumstances, and above all when the security of the state was threatened, the need for urgent information could outweigh normal reticence concerning the rights of masters. Leonhard Schumacher painstakingly reconstructs the pattern of these exceptions, and thus determines, through an ingenious test, how significant the countervailing public interest had to be before the rights of slave owners were infringed.

As Cicero tells us, the late Republic recognized two broad areas of public law in which the testimony of slaves could be sought. One involved sacrilegious sexual conduct, as for instance involving a Vestal Virgin; the intimate nature and dreaded consequences of such crimes made it necessary to obtain all available testimony. The other area involved conspiracies directed against the state or endangering public order: not only attempted revolutions such as Catiline's, but also major criminal conspiracies such as the murderous activities concealed by the rites of Bacchus (187 B.C.) or the assassination of the urban praetor by a mob of angry moneylenders (89 B.C.). But otherwise, it was not possible, even in the case of serious crimes such as murder, for slaves to testify publicly either for or against their masters, although private hearings were sometimes conducted to clear up disputed matters.

To some extent this typology endured in the empire. But its ambit tended to increase as criminal trials for *maiestas* (a flexible political offense against the state's dignity) were also admitted as a protection for the emperor and his family. In A.D. 16 Tiberius overcame any remaining scruples about accepting servile testimony by forcing the accused to convey his slaves to an agent of the state, who then ques-

tioned them under torture; a patent intrusion into property rights was in this way thinly disguised. The path to this "solution" was foreshadowed in Augustus's legislation on adultery (18 B.C.), which prescribed that defendants' slaves should be heard to testify and then, regardless of the outcome, made public property; if a defendant was acquitted, the state paid compensation. This procedure ostensibly ensured unbiased testimony about intimate household affairs.

The most controversial imperial extension apparently occurred in A.D. 39, when Caligula admitted the testimony of slaves against their masters for fiscal crimes as well, perhaps especially the concealment of taxable assets. This measure, highly unpopular among the aristocracy, nonetheless remained in force after Caligula, although its sweep was eventually limited. The final legal situation is summarized in a rescript of Septimius Severus from 196: questioning of slaves is permitted only in cases of adultery, census fraud, and *maiestas*.

Although this topic has been much discussed in recent years, Schumacher renders all earlier treatments obsolete. His monograph is a model.

BRUCE W. FRIER
University of Michigan

MARTIN GOODMAN. *State and Society in Roman Galilee, A.D. 132-212*. (Oxford Center for Postgraduate Hebrew Studies.) Totowa, N.J.: Rowman and Allanheld. 1983. Pp. x, 305. \$34.50.

This book, an Oxford doctoral thesis, has two distinct halves. In the first (chaps. 3-6) Martin Goodman discusses Galilean society in the second century of our era: population, relationship of Jews to gentiles, and village life. He draws on rabbinic as well as nonrabbinic sources and demonstrates that Galilean villages resembled the contemporary villages of Syria. The second part of the book (chaps. 7-10) is far more interesting and important than the first. Goodman attempts to locate the position of the rabbis within Galilean society by delineating the limits of their legal power. Rabbinic texts presume that the rabbis had judicial authority, but Goodman concludes that the rabbis did not obtain support from the Jewish masses until the third century and did not obtain recognition from the Roman government until the fourth. This conclusion is supported by an analysis of the legal cases recorded by rabbinic literature. The theoretical reach of the rabbis far exceeded their actual grasp. Rabbinic jurisdiction competed with the municipal and the imperial, and Goodman's discussion of the relationship of local law to Roman law, and local jurisdiction to imperial jurisdiction, should interest all historians of Roman antiquity.

The two halves of the book share an important methodological premise: the only reliable sources for the activities of the rabbis in the second century are the rabbinic texts that were redacted no later than the third century (notably the Mishnah). Goodman therefore tries to avoid basing any argument on either the Babylonian or the Palestinian Talmud. The merits of this laudable procedure are offset by Goodman's frequent failure to adhere to it: for example, the argument (pp. 108, 160) that second century rabbis were "superhuman holy men" is based on third century ideology, as Goodman's own footnotes demonstrate. Goodman does not explain why he relies on handbooks that do not share his skepticism toward the value of later documents for a reconstruction of second century Jewish society: For example, Goodman endorses the old view that most second century rabbis were artisans, but is unable to cite support from the texts that he deems reliable (pp. 93, 109). More disturbing are the occasional howlers: Goodman does not know that *ger* in rabbinic Hebrew means "son of a proselyte" as well as "proselyte" (p. 44, n. 58); *beth aba* is not the name of an otherwise unattested village but "the house of my father" (pp. 126–27, 157); and not "disasters inflicted upon Israel by the gentiles" but "punishment of the gentiles by God" (p. 148, n. 231). Relevant rabbinic evidence is sometimes overlooked: note 24 on page 42 ignores Mekilta Pisha 15 (Lauterbach's edition pp. 119–20); Goodman's certainty about the nonobservance by the masses of the laws of tithing and the sabbath-year (p. 103) should have been tempered by Tosepta Demai 5:2 (Lieberman's edition p. 85) and Erubin 5:10 (Lieberman's edition p. 113).

A brief review is not the place for a full presentation of errors of this sort, but I have cited enough to demonstrate that Goodman's text cannot be trusted without checking the references and without searching for missing evidence. We have the right to expect better from an Oxford dissertation co-directed by the editor of the *Journal of Jewish Studies*. On the positive side, however, let us not be ungrateful for what the author has accomplished. His questions are excellent and his answers are convincing.

SHAYE J. D. COHEN

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JO ANN MCNAMARA. *A New Song: Celibate Women in the First Three Christian Centuries*. (Women and History Series.) New York: Institute for Research in History or Haworth Press. 1983. Pp. 154. \$29.95.

This study, concerned with the role of women in the Christian church during the first three centuries, proposes the following interesting, if not fully original, thesis: "The virginal ideal was not imposed on

women by men fearful of their own sexuality. It was a revolt against an earlier set of male definitions" (p. 5). All agree that female as well as male monasticism was first a spontaneous, unofficial, and even controversial movement. But we know less about the motivations or functions of celibate women in earlier centuries. Any contribution to the question should therefore be welcome. Many, like the present reviewer, will probably open this book with expectation but will be sorely disappointed.

To conduct a useful task in the area of early Christianity, one needs more than enthusiasm, determination, or sympathy for the subject. Other ingredients, including familiarity with the history and literature of the period, are also needed. Granted that old sources can be submitted to a new analysis, the endeavor still requires respect for facts and texts. In this instance, no clear division is ever made between successive periods or between East and West (essential in a case like the role of the deaconesses). Spurious writings are regularly used without concern for their genre, their origin, or their date. The reader retains the definite impression that Jo Ann McNamara has only read into the documents, without any verification, a wealth of recent hypotheses. We find, page after page, affirmations based on suppositions, possibilities, and generalizations. Inaccuracies abound.

An example of the way sources are used are references to Clement of Rome. The author refers four times to the "undisputed letter to the Corinthian community" (p. 60), strangely mentioned in the footnotes as "Address to the Greeks." The most amazing example is what we are supposed to learn from chapters 4 and 40, about Clement: he is "one of the first to call for an end to women's ministerial functions," he "rebuked wives who committed sedition against their husbands," he ordered the services "to be conducted in a proper and settled manner, by appointed persons in the temple," he "wanted to see his congregations break away from the house churches" (p. 60). But we are also told that, despite Clement's ideals, "most congregations did not have temples and did not enjoy the services of a settled clergy" (p. 74)!

Just as puzzling is the way the spurious works transmitted under the name of Clement are quoted together with documents of the late first century. McNamara concedes that the two *Letters to the Virgins* cannot be "securely" used "for evidence on the exact regulations of the virginal life in the late first century," but she is still convinced that "they attest to an attitude that was certainly developing then and that continued to hold sway for the next century" (p. 58). And not only are the texts cited from the wrong period, they are also wrongly interpreted (see pp. 24, 52, 73–75). The *Homilies* are mentioned together with Polycarp, Hermas, and Ignatius, as a "tradi-

tional account of wifely love" (pp. 52, 74–75), while in their present form they may not belong at all to the first three centuries. This is still more evident of the apocryphal letter of Ignatius of Antioch to the Virgin Mary that is taken seriously by McNamara: "Among the scraps of memorabilia preserved by first-century Christians is a letter attributed to Ignatius of Antioch, addressed to the leading Christian widow of the age, the Virgin Mary herself" (p. 58).

One does not have to agree with everything to read a book with pleasure and for profit, as verified by E. Schussler Fiorenza who has also recently presented a feminist reconstruction of Christian origins, *In Memory of Her* (1983). But an essay that consistently ignores the most elementary laws of scholarship deserves oblivion rather than discussion.

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MEDIEVAL

GEORG SCHEIBELREITER. *Der Bischof in merowingischer Zeit*. (Veröffentlichungen des Instituts für Österreichische Geschichtsforschung, number 27.) Vienna: Hermann Böhlau. 1983. Pp. 312. S 92.

This study of the Frankish episcopate with some references to Visigothic and Lombard prelates, covering the period from the fifth to the eighth centuries, was conceived as a contribution to "histoire des mentalités" (p. 267), and specifically as a contribution to the discussion surrounding the question of the continuity of antiquity and the often obscure transition from late antiquity to the early Middle Ages. The bishop is used as a paradigm. Georg Scheibelreiter therefore felt justified in omitting as much as possible not only references to the political and constitutional position of the bishop but also to episcopal spirituality. Scheibelreiter intends to discuss the latter in a future study, but as a result of these significant omissions, in a book that ostensibly has the bishop as its topic, the Merovingian bishop remains an abstraction for the time being, despite a prodigious amount of research. Perhaps the bishop was, as the author claims, in most if not all respects representative of the period discussed, its mentality, and "genre de vie" (p. 19), but he remains curiously ill-defined. Only a single dimension of his mentality is repeatedly brought out in connection with much detailed factual evidence—his presumed consciousness of noble descent. Accordingly, Scheibelreiter emphasizes only those aspects that he sees as expressing a noble way of life. "Der Heilige ist grundsätzlich ein grosser Herr" (p. 22). Everything else is relegated to the far background. This includes, for

example, so important and influential a figure as Columbanus (mentioned only twice in passing) and the missionary bishops. Scheibelreiter sees nobility and sanctity as closely intertwined during the Merovingian period until later, under the Carolingians, the figure of the "Adelsheilige" (the influence of Karl Bosl and Friedrich Prinz is pervasive throughout the study and accordingly acknowledged) is no longer understood and gives way to a conception of the bishop that was derived from canonical and theological principles (p. 23). Under the circumstances, it is regrettable that Scheibelreiter refuses to discuss his understanding of the term "noble" in the meaning of "noble by descent."

The main sources for the book are Merovingian saints' lives and, naturally, the writings of Gregory of Tours. The work is divided into seven chapters corresponding to the stages of life ordinarily described in the *vitae* of saints: birth and origin (discussed extensively), education, preparation for the office, installation/consecration, social and organizational tasks, journeys, and death of the bishop. A final summary is followed by a table of abbreviations, an index of secondary literature cited, an index of personal and place names, as well as a brief general index. The book is a treasure-trove of factual, often very interesting, details that the author culled from his sources and presents almost in catalogue form in the relevant chapters. The diligence of the author is admirable. But partly because of the lack of a thematic framework (chapter 4 is the exception) and partly because of a very narrow approach to the primary sources and his one-sided interpretation, the author's main thesis is not entirely convincing. One looks forward to Scheibelreiter's future studies, which promise to provide a more complete picture of the bishop in his diocese.

UTA-RENATE BLUMENTHAL
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ROSAMOND MCKITTERICK. *The Frankish Kingdoms under the Carolingians, 751–987*. New York: Longman. 1983. Pp. xiv, 414. \$19.95.

The publisher regards this as "the first modern book in any language to examine the entire history of the Carolingian 'dynasty' in western Europe in its full social, political, and cultural context." Rosamond McKitterick's preface narrows these horizons after 843 to "more or less . . . the west Frankish kingdom" (p. ix). Within that scope she has made her synthesis topically selective. The result is an often original, challenging book, whose main strength is its many scholarly excursions rather than sustained, comprehensive synthesis of the sort found in the Glotz volumes or, more recently, in

Susan Reynolds's sharply etched overview of medieval English towns. Thus the tenth-century, west Frankish king, Lothar, bulks larger than Emperor Lothar, whose magnificent portrait (unfortunately printed in reverse on the cover) galvanizes the reader's attention.

The three chapters on Frankish culture are outstanding. They emphasize the high quality of Carolingian artistic and musical invention and bring organization and precision to the outlying generalizations of specialists in these fields. There is also fresh delineation of the individual accomplishments of the major cultural centers, the lively interaction and influence of these centers, and, for several of them, their vigorous continuation in the supposedly dark tenth century.

The author significantly enriches our knowledge of familiar topics. She brings less familiar ones, like Scripture scholarship and theological inquiry, into intelligible focus. In *The Frankish Kingdoms* Aristotle takes on new life in the tenth century. The originality of John the Scot becomes even clearer as his lively subsequent influence on others is revealed. Equally enlightening is the author's full, nuanced account of the Carolingian reshaping and "canonization" of Benedictine monasticism. She shows that Cluny was not an isolated rescue operation in a sea of monastic corruption but one late and especially vigorous emanation in a series that began with Benedict of Aniane. In all of this, only a historical traditionalist will remain blind to why many who have closely studied this period regard it as the fundamental renaissance of European history.

Although often valuable, the author's synthesis of political and social history is less consistently successful. Certainly Louis the Pious, Charles the Bald, and the tenth-century Lothar deserve her positive revisions. Unfortunately, some revisionist overkill distorts the religious initiatives of Pepin III. The political and social outlook of Charlemagne also suffers some foreshortening. And the revisionists concerned with the Viking raids are not given a full hearing. Such deficits often result from ignoring or slighting earlier studies. Neglect of F. L. Ganshof's *Feudalism*—a neglect seemingly now *de rigueur* among many medievalists—and of many of his special studies, most notably of the programmatic capitularies of 802 and 817, produces serious misunderstandings. Ferdinand Lot's *Naissance de la France* and Lucien Musset's and Renée Doehaerd's volumes in the *Nouvelle Clío* series, among other earlier works, would have repaid serious attention. Such oversights and a tendency to lapse into awkward expository order and style, if frustrating, do not on balance prevent this book from being a major publishing event.

WILLIAM M. DALY
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W. LOURDAUX and D. VERHELST, editors. *Benedictine Culture, 750-1050*. (Mediaevalia Lovaniensia, first series, number 11.) Louvain: Leuven University Press. 1983. Pp. xiii, 239. 1.490 F.

The most recent volume in the Louvain series on medieval studies gathers international contributions describing "the 'Benedictine Age,' stretching from the Carolingian renaissance to the reform movement initiated by Gregory VII" (p. vii). Although most of its contents bear on monastic customs or intellectual life, John McCulloh's article on historical martyrologies and Ludolf Kuchenbuch's on the twelfth-century land register of the abbey of Prüm stand apart as essays on source materials. McCulloh's discussion of martyrologies points out that these texts deserve more scholarly interest than they have heretofore received because they indicate their compilers' assessments of the relative importance of individual saints. Kuchenbuch's meticulous definition of a wide variety of forms of manorial obligation among Prüm's possessions expands on the *annaliste* Charles-Edouard Perrins's earlier study of the abbey's register. Kuchenbuch identifies a tendency away from servile labor rents and toward money payments. This challenging implication of an economic shift in favor of late tenth-century peasants will require confirmation among comparable source studies.

The articles of Josef Semmler, Adriaan Bredero, Friedrich Lotter, and Pierre Riché are closely related. Semmler's exploration of Benedict of Aniane's attempt at standardization of Carolingian monastic customs leaves little doubt that it failed. As Semmler points out against Philibert Schmitz's old argument, Benedict of Aniane indeed insured the ascendancy of the earlier Benedict's rule, but no similarly uniform custumal accompanied its promulgation.

Bredero's study controverts the similarly conventional representation of Cluny as a foundation unusual in the degree of its liberty from feudal and episcopal ties. He shows that the abbey was typical of Carolingian monastic reform, both in terms of its foundation charter and its subsequent functioning.

Although Cluny may thus have been structurally conservative, according to Bredero, Lotter's discussion of one element in its hagiography suggests that at least its outline for lay piety was substantially new. Lotter argues that the abbot Odo's life of the noble saint Gerald of Aurillac represents the transformation of Cluniac spirituality into a normative ideal for laymen. Both Bredero's and Lotter's arguments find general support in Barbara Rosenwein's recent study, *Rhinoceros Bound: Cluny in the Tenth Century*.

Riché's account of monastic education for lay children as well as oblates continues the material of his own magisterial study, *Education and Culture in the Barbarian West*, and, like Lotter's contribution to

this volume, describes the influence of monasticism on lay culture. Riché shows that the black monks' benevolence and thoroughness made their child rearing so attractive that reformers were unable to exclude laity.

Finally, Carlos Steel's and Maria Ludovica Arduini's contributions consider monastic intellectual life from the point of view of its leaders rather than its transient participants. As Steel emphasizes, in his essay on John the Scot's rationalism, the author was a nonmonk whose work cannot be understood outside the heavily monastic context of Carolingian thought. Yet Arduini emphasizes that the terms *auctoritas*, *traditio*, and *ratio* were inextricably related for monastic intellectuals such as Anselm, Rupert of Deutz, and Adelard of Bath, who have often been seen as harbingers of a scholastic future. Her conclusion that categorization of Benedictine writers as prescholastic distorts their self-understanding is compatible with Steel's minimization of the artificial distinction between earlier monastic and nonmonastic authors.

On the whole, *Benedictine Culture, 750-1050* offers a discussion of early medieval monasticism highly characteristic of contemporary interests; its articles present monks' contributions in their widest social and intellectual contexts. The volume's attention to the history of Cluny and the interaction of monasticism with noble laity is of especially timely interest. The editors' choice of several related papers mitigates the looseness of topical structure inevitable in a *Festschrift*. Specialists in monastic culture will find much of interest here and some genuinely new points of view.

CAROL NEEL
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PAUL H. FREEDMAN. *The Diocese of Vic: Tradition and Regeneration in Medieval Catalonia*. New Brunswick, N.J.: Rutgers University Press. 1983. Pp. viii, 230. \$20.00.

Although the cathedral archive of Vic is immensely rich in medieval documents, it has been strangely neglected by non-Catalan historians. Now, however, Paul H. Freedman has used it, and the Archivo de la Corona de Aragón, to study the evolution of the church of Vic in the eleventh and twelfth centuries. He examines first its rise, its acquisition of lordships and wealth, and its links with the counts of Barcelona from 866 to 1099; he shows clearly that the church became rich and powerful not to compensate for any weakness of comital authority, but precisely because the counts endowed and supported it, working in collaboration with the bishops to resettle the area and, from 1089 to 1099, to restore the metropolitan see of Tarragona.

The failure of the latter scheme is linked by Freedman to Almoravide aggression and anti-French sentiment, following Santiago Sobrequés Vidal, although he does not explain why he prefers the latter's views to those of Lawrence J. McCrank, and one might suggest that El Cid's death (1099) was also significant in removing the main barrier between the Almoravides and Tarragona. Whatever the reason, the counts seem to have lost interest in the church of Vic; and Freedman devotes most of his pages to examining the functioning of the cathedral chapter in the twelfth century, its relations with the city of Vic, the extent to which the bishops controlled the countryside through their castles, and, finally, the lawsuits in which they were involved.

Clearly, this is a study of the church's legal and political power; it excludes most spiritual and economic considerations. Freedman argues that it is of interest in spite of the fact that Vic was now far from the supposed dynamism of the Muslim frontier; but the defensiveness is unnecessary, for the evaluation of the church of Vic is no less interesting than that of, say, Palencia or Chichester. This is a very valuable and detailed study of many aspects of ecclesiastical lordship. It is particularly informative on relationships with the local gentry and the way in which the diplomatic forms of lawsuits and verdicts really concealed delicately negotiated compromises in a system only superseded, apparently, when the sovereign began to exercise direct authority again in the region after 1196.

The work provokes minor questions, of course. Were territorial archdeacons known anywhere in Spain before 1100 (p. 49) or indeed in Vic? For even as late as 1280 the *Rationes Decimarum Hispaniae* fails to mention them. And, although Pere de Cardona may have enjoyed a canonry in Vic cathedral from 1167 to 1181, as a prominent member of the Castilian royal chancery he must have been nonresident, at least from 1178. But these are trivia in what is undoubtedly the best study in English of any medieval Spanish see and a considerable contribution to the history of both the church and feudalism in the High Middle Ages. Freedman is to be congratulated not only on his scholarship but also on the book's admirable presentation with maps, plans, and genealogical tables by his publishers.

DEREK W. LOMAX
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FRANK BARLOW. *William Rufus*. (English Monarchs.) Berkeley and Los Angeles: University of California Press. 1983. Pp. xix, 484. \$24.95.

In 1882, when Edward Augustus Freeman published the two volumes of his *Reign of William Rufus*,

he admitted that William's thirteen years on the throne could be viewed as "a kind of episode" between the reigns of stronger monarchs. Further, he stressed that even though his protagonist had many faults as a ruler, the principal one resulted from his failure "to reconcile the English nation to the Norman Conquest, to nationalize, so to speak, the Conquest and the dynasty which the Conquest had brought in." Ultimately, of course, Henry I was to achieve that reconciliation, thereby assuring his own high place in history, so it follows that the peculiar tragedy of Rufus's life lay in the fact that he himself "might have held that place, if he had been morally capable of it" (vol. 2, p. 456). And if one wonders about the precise nature of this moral incapacity, Freeman's answer is unambiguous, although somewhat shrouded in pious Victorian rhetoric: "What makes William Rufus stand out personally in so specially hateful a light is . . . indulgence in the foulest . . . vices of heathendom . . . the vices of the East, . . . the habits of the ancient Greek and modern Turk" (vol. 1, pp. 147, 157, 159).

Frank Barlow's *William Rufus* is the first study to appear since 1882, and its very language demonstrates the changes a century can bring. Thus the king's court has now become a quite explicitly homosexual "hotbed" of sodomy, although in the case of Rufus himself, "[o]n the whole the evidence points to . . . bisexuality" (p. 109). Still, this new frankness scarcely alters the alleged facts. Even in the case of the reign's place in English history, Barlow's judgment seems little different from Freeman's insofar as he argues that its importance results largely from a negative reality—"that it prevented the reign of Robert Curthose and also assured the reign of Henry I" (p. 433).

Nevertheless, for all their similarities, the two works are as different as night and day. In effect, Freeman's judgments are moral whereas Barlow's are historical. If, for example, the one abhors the king's swearing as "a form of irreligion which startled not only saints but ordinary sinners" (vol. 1, p. 147), the other calmly explains not only the origins of William's favorite curse but also the likely bias of saints. Similarly, where the one merely condemns sodomy, the other ranges widely in both history and literature to present the sexual realities of bachelor life within an overwhelmingly military society. Lastly, where Freeman sees little more than tyranny and (with the aid of Ranulf Flambard) the imposition of the feudal system, Barlow sees only the normal governmental techniques of the age, rather likes Flambard, and never mentions feudalism at all.

Further distinguishing Barlow's work is not just a deeper knowledge of the age but also a keen appreciation of how that knowledge can be used to enrich the study of kings. As Freeman understood William Rufus and his world, the only things that

mattered were religion and politics, law and the constitution. Barlow takes a different view. His three appendixes may contain only narrowly technical details—on the royal itinerary, the names of sheriffs, and (here updating the work of D. C. Douglas) even on what is known about the children of William I and Matilda. But his text, undergirded by an extensive bibliography, ranges much more widely. He tries to understand William II's likely values by exploring how children were reared in royal and high baronial families, by using the *Constitutio Domus Regis* of the following reign to enrich our understanding both of kingly finance and of an itinerant court life, and, not least of all, by presenting a remarkable understanding of the medieval hunt in order to show that the king's much-debated death was much more probably a hunting accident than the murder it is so often alleged to be. In short, Barlow's *William Rufus* is as close to a definitive study as we are ever likely to get and surely a worthy addition to California's still-continuing English Monarchs series.

CHARLES T. WOOD
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THOMAS K. KEEFE. *Feudal Assessments and the Political Community under Henry II and His Sons*. (Publications of the UCLA Center for Medieval and Renaissance Studies, number 19.) Berkeley and Los Angeles: University of California Press, 1983. Pp. ix, 291. \$29.50.

Contemporary chronicles, oft-told medieval legends, and modern historians are inclined to agree that the Angevin kings were brutal exploiters of *all* their potential sources of revenue in England and on the Continent. New as well as traditional resources were regularly put to the sword of fiscal and feudal exaction, without ruth or remorse. Among such royal graspers, Henry II (1154–89)—the greatest of the lot—was the most innovative at picking other men's pockets. High tax assessments, based on a cruel ratio of service demanded to land held in fee (and as divided into the component units of knights' fees), were the bread-and-butter of the exploitation, while scutage ("shield money," the cash taken in lieu of personal or direct military service) added the occasional dollop of jam for the voracious maw of the Exchequer. This is, at least, the explanatory framework in which the popular view of bureaucratic, cruel, and unpopular Angevin monarchy is regularly placed.

Thomas K. Keefe offers a significant revision, brief but well focused. He argues that Angevin monarchy was not posited on a systematic difference of interest between the king and his great nobles.

The call to royal military service was assessed at a modest or reasonable rate—almost always below the aggregate of the subinfeudated knight service the magnates themselves were able to extract from their fiefs. The feudal levies were rarely summoned at, let alone beyond, the full letter of the accepted law. Nor was scutage systematically or regularly exacted, let alone at a rate beyond that agreed on for personal, rather than for cash-commuted, service. Keefe mainly confines his analyses to data adaptable to tabular, quantitative, and statistical analysis; much of his material is designed to illuminate such specific issues as the rate of assessed service imposed on different levels of magnates or on lay and ecclesiastical lords and the frequency of the imposition of feudal aids and scutage between Henry II's accession and the death of John in 1216. His argument is cogent and presented with a judicious touch that rests on extensive documentation and calculation.

This project began as a dissertation, and in many ways its nominalist genesis still shows. Many of its valuable tables and appendixes beg for more analysis instead of, or in addition to, the elaborate lists of men that are often presented. Table 21, on forest fines (pp. 130–31), is a case in point: thirty-five men who paid all or part of their fines are listed, but we get little in the way of summary or conclusions regarding either the men or the king's policy. This is a small return for the considerable labor Keefe has expended in collecting the information. The material in the four appendixes is likewise too raw—too much effort has been expended in extracting and presenting it and not enough in discussing its significance.

These are slight flaws given the merit and importance of this book. But they should remind the author that once he has established his right to the floor—as he has certainly done here—he should not yield it again until he has told us even more about this field in which he will surely continue to make a mark. This is a good technical monograph on an important, technical subject.

JOEL T. ROSENTHAL
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JAMES W. ALEXANDER. *Ranulf of Chester: A Relic of the Conquest*. Athens: University of Georgia Press. 1983. Pp. xi, 191. \$16.50.

James W. Alexander's biography of Ranulf of Chester reminds us that a half-century has passed in this field of American medieval historiography, for it was in 1933 that Sidney Painter published his Yale doctoral dissertation on William Marshal and demonstrated the feasibility and profit of biographical

study of nonroyal lay figures of the late twelfth and early thirteenth centuries. Of course, it has always mattered whom one chose to write about. Painter had the assistance of a rhymed chronicle of 19,000 lines devoted exclusively to his subject. Having no comparable centerpiece to work from, Alexander has put his study together with greater difficulty from the sparse comment of chronicles, charters, and fiscal documents. And because Ranulf is one of the two or three figures of his age most likely for such treatment, it is fair to say that any attempt to produce book-length studies of his contemporaries—earls like Richard de Clare, William de Ferrers, and Hugh Bigod—is foredoomed to failure.

Descended from the Norman earls of Chester and next to the last of the line to hold that office, Ranulf was a significant figure in England for forty years, briefly serving Henry II and then Henry's two sons and grandson. Two features of his political career stand out—his loyalty in turn to each of the four kings he served and the material profit that accrued to him from such loyalty. Although his support of Richard Lionheart against John, and of John against his barons, was important if not crucial, his role as loyalist during the minority of Henry III accounted in large part for the survival of the young king and in turn raised the earl to new heights of material success. Earl of Chester and earl of Lincoln, he was lord by his death in 1232 of more than 250 knight's fees in and out of Cheshire.

Well written and interesting, Alexander's study of the earl offers little to quarrel about. He seizes the opportunity to pull together most of his persuasive arguments, some expressed on earlier occasions, for the view that Chester cannot be called a county palatine during the life of Ranulf. Both the term and the concept were absent in the early thirteenth century, and evidence that they did exist appears only toward the end of the century. In the absence of norms and models, however, Alexander's insistence on the earl's pinchpenny patronage of religious houses does not convince, especially when he adduces the opinions of monastic historians that the great age of monastery-founding had passed in the early thirteenth century and enumerates some fifty grants of lands, rents, quittances, immunities, and judicial rights made by Ranulf to abbeys, convents, and hospitals in England and Normandy.

Certain addenda would have greatly increased the visual image of Ranulf in this thirteenth-century world, and the most fundamental would have been a detailed genealogical chart, valuable out of all proportion to the bother involved in compiling it. Not required but helpful would have been a map of England showing the earl's lands, a need partly filled by the author's feodary of Ranulf's holdings.

FRANKLIN J. PEGUES
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MARK BUCK. *Politics, Finance, and the Church in the Reign of Edward II: Walter Stapeldon, Treasurer of England*. (Cambridge Studies in Medieval Life and Thought, third series, number 19.) New York: Cambridge University Press. 1983. Pp. ix, 253. \$49.50.

Walter Stapeldon rose from a fairly modest family background to a successful career in church and state. The status of Walter's father seems to have been that of a freeholder in Devonshire, and the opportunity for Walter to move beyond his social origins was based on his education at Oxford. Walter was a learned man who valued education, and during his lifetime he founded Exeter College, Oxford, and endowed a grammar school in Exeter. Stapeldon began his ecclesiastical career in his own diocese and was by 1301 a canon of Exeter cathedral. In 1308, following a disputed election, he became bishop of Exeter, an office he held until his death. Exeter was not a wealthy diocese, but Stapeldon energetically solicited funds and contributed substantially in his own right to the ongoing project of rebuilding the cathedral church; he contributed as well to the maintenance of religious houses, parish churches, and bridges in his diocese. Bishop Stapeldon also compiled a responsible, active record in carrying out his pastoral and administrative duties.

As a bishop, it would be expected that Stapeldon would have become involved in governmental service. Such was indeed the case, but for a decade Stapeldon participated in secular government in only a limited fashion. He held firm in matters of clerical privilege, but he avoided identification with any of the political factions of the middle years of Edward II's reign. He regularly participated in parliaments; twice in 1313 and again in 1315, 1316, and 1319 he was part of diplomatic embassies sent to Paris to discuss matters, particularly Gascony, with the king of France, and in 1318 he went on a diplomatic mission to the Low Countries. Through these activities the bishop may have gained the confidence of the king, but, as Buck suggests, the paucity of evidence concerning the development of Stapeldon's political career makes his appointment by the king in February 1320 to the treasurership of England seem somewhat abrupt. Buck sees in the appointment the growing influence of the younger Hugh Despenser. Stapeldon ceased to be treasurer after nineteen months, leaving office for unexplained reasons, but he was reappointed by the king in May 1322. This second term as head of the Exchequer lasted until July 1325, and Buck speculates that it may have ended in order to free Stapeldon to commit more time to international diplomacy. A notable achievement of Stapeldon's second treasurership was the development of a

calendar of diplomatic and other documents, which is named for him—a project that was itself part of a larger effort of sorting and calendaring documents. There was considerable zeal given to reforming Exchequer procedures during the years within which Stapeldon's treasurerships fell (Buck's article in *The English Historical Review* 97 [1983] needs to be consulted together with his book), with the aim of increasing the king's revenues. Stapeldon's reputation seems to have been compromised by his association with rapacious royal policies, but Buck finds the evidence equivocal as to whether or not Stapeldon personally engaged in profiteering while in office. Stapeldon's murder on October 15, 1326, at the hands of a mob that captured him as he fled toward sanctuary in St. Paul's Cathedral clearly indicates his standing with a segment of London's population.

As a bishop, a major officer of state in Edward II's reign, a patron of education, and the first English bishop since Thomas Becket to be murdered, Stapeldon is a worthy subject of study. Buck's title suggests, however, that the bishop proved to be elusive quarry. Buck's narrative consists of a series of episodes in which Stapeldon appears sometimes clearly, but always moving in and out of the shadows. This is not to castigate Buck, who tells us what the evidence will allow and in the process makes a meaningful contribution to the history of Edward II's reign.

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ROBERT S. GOTTFRIED. *The Black Death: Natural and Human Disaster in Medieval Europe*. New York: Free Press of Macmillan. 1983. Pp. xvii, 203. \$16.95.

The Black Death of 1348 and its subsequent manifestations over the succeeding 300-year period have been the subject of controversy for the past few decades. The traditional view put forth by F. A. Gasquet in 1893 of a devastation so great that it marked the end of the Middle Ages was challenged by Marxists, such as Eugenii Aleksevich Kosevskii, who argued that the plague was simply one additional factor in the general crisis undermining European rural economy and society and hierarchical structures. It was also challenged by bacteriologists such as J. F. D. Shrewsbury, who held that the plague bacillus was not so virulent as historians had previously assumed. In recent years, however, a number of detailed regional studies by scholars from various parts of the Western world, such as David Herlihy in the United States and Elisabeth Carpentier in France, have reemphasized the effects of the plague.

Robert S. Gottfried's study summarizes the conflicting theories and recent studies and manages to come out with an explanation incorporating the views of both Gasquet and Koseminskii, as well as incorporating the generalizations of scholars such as William McNeill. Gottfried is able to do this, because he looks at the plague from an environmental and ecological framework, documenting demographic changes and assessing the social, economic, political, and intellectual implications. The result is a fascinating study that will cause historians to reexamine many of their assumptions about European society and provoke many debates.

What Gottfried has managed to do is propose the same kind of grand thesis for the later medieval—early modern period as Pirenne did in his explanations of the effects of the Arab conquest of the Mediterranean. Although Gottfried carefully dates the beginning of a decline in economic conditions prior to the plague, and thus supports Koseminskii, the plague, he maintains, not only accelerated the changes but also affected the nature of change. The result was a weakening of the church, the increased use of secular languages, a change in intellectual outlook brought about by the undermining of scholastic methods, the development of the scientific method, greater power to the lower classes in the West and increased serfdom in the East, the weakening of the Muslim hold on the Balkans, a change in the nature of Western medicine, as well as any number of other things.

Gottfried, however, is cautious in his claims, understating his rather radical generalizations and always careful to cite contrary views and to raise objections. Still his generalizations remain. Although I am not fully convinced of his arguments, they have caused me to do a lot of rethinking. Even in my own specialty, medieval medicine, he has used my own data to formulate new insights. More than this one cannot ask of an author.

Although the book is an alternate selection of the Book of the Month Club, it is a scholarly work, well footnoted, and with an excellent but not exhaustive bibliography that utilizes most of the standard primary and secondary sources. Gottfried, however, wears his scholarship lightly. The result is an interesting, provocative study, which should be debated for many years to come.

VERN L. BULLOUGH
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JOACHIM-FELIX LEONHARD. *Die Seestadt Ancona im Spätmittelalter: Politik und Handel*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 55.) Tübingen: Max Niemeyer. 1983. Pp. xii, 506. DM 136.

Joachim-Felix Leonhard's *Die Seestadt Ancona im Spätmittelalter* deals with the history of the Italian Adriatic seaport of Ancona between the beginning of the First Crusade (1096) and the end of the pontificate of Boniface IX (1404). Its subtitle, *Politik und Handel*, should be more properly *Aussenpolitik und Handel*, as the work devotes three of its four parts to the relation of Ancona with the major powers, the German and Byzantine emperors, and the popes; with the Adriatic ports and particularly Venice; and with the other localities of its region, the so-called *Marche*. The last part is devoted to the commerce of the city. The internal affairs of the city are not dealt with, except incidentally. This limitation is due to the nature of the sources. Significant archival material survived at Ancona only from the fourth quarter of the fourteenth century, so Leonhard had to base his study largely on outside sources, and in these, information on the foreign policy and trade of Ancona predominates.

Leonhard traces chronologically the relations of the city with outsiders, big or small, assessing critically the present state of scholarship where necessary. As he bases his account on much unpublished material in Italy and Dubrovnik, as well as on the published sources, there results a trustworthy, very detailed study of the complex foreign relations of Ancona. The reading is not easy, and the book might have benefitted from further editing. For instance, the point that "Guelph" and "Ghibelline" are very much shifting party labels is made several times. Yet Leonhard's analytical presentation will prove indispensable for anyone working in his area and period. Ancona appears from his study in a second-level position, used and abused by the major powers and unable to assert itself against Venice. It navigates, however, sufficiently well between the different parties to retain a degree of autonomy in developing its natural advantage as the best-located Adriatic port of Central Italy for traffic with Dalmatia, North Africa, and the Eastern Mediterranean.

The commercial relations are presented in geographical order. Leonhard examines Ancona's connections with other parts of Italy, with Dalmatia, with the Byzantine Empire and the Orient, and with the Western Mediterranean and Europe north of the Alps. His treatment is synthetic and very informative, a good basis for further, exhaustive studies. In the future the author intends to investigate the commercial relations between Ancona and Dubrovnik in the fourteenth century and at the beginning of the fifteenth century in more detail (p. 8, n. 48). He also plans to publish some of the sources he used for these two cities (p. 315, n. 245).

The scholarly usefulness of Leonhard's work is enhanced by a note on the march of Ancona and its margraves in the twelfth and thirteenth centuries; by three appendixes listing respectively dates for the

office of consul and *podestà* in a number of localities in the *Marche*, the itinerary of the papal legate Cardinal Rainer of Viterbo, and Anconitans as officials in two other communities; and by the publication of fifty-five documents, including a number of papal enactments.

REINHOLD SCHUMANN
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GÉRARD SIVÉRY. *Saint Louis et son siècle*. (Figures de Proue.) Paris: Tallandier. 1983. Pp. 672. 120 fr.

It is difficult to do justice in the limited space allotted here to Gérard Sivéry's massive new study of the reign of St. Louis. The author, a specialist in the social and economic history of medieval Flanders and Hainaut, has exhaustively examined the available published and unpublished sources for the saintly king's rule. Although the scope of the book is comprehensive, including brief surveys of the cultural developments that took place during the central decades of the thirteenth century, Sivéry's main focus remains the institutional and administrative history of St. Louis's reign, and it is in the analysis of the instruments and conceptions of royal government that his study makes its most important contributions. Sivéry stands, in effect, as Louis IX's first administrative biographer.

Beginning with a description of the governmental *équipe* bequeathed to the young king by his grandfather, Philip Augustus, Sivéry traces the progressive implementation of new models of rulership and new administrative procedures that slowly transformed the nature of French kingship over the course of the thirteenth century. With the abrupt end to the brief rule of his father, Louis VIII, in 1223, Louis IX became heir to a realm profoundly troubled by the consequences of forty years of royal expansion and centralization. The realm, moreover, was already internally divided into economic zones of widely disparate nature, with the northern and northeastern regions enjoying both the benefits and disturbances brought about by the rapid progress of the new commercial economy, largely absent in other parts of France that remained rural in economy and mentality. In what is perhaps the most fascinating chapter of this huge work, Sivéry minutely examines the findings of the *enquêteurs* whom Louis, preparatory to his departure on crusade in 1248, had delegated to journey throughout the realm to hear and adjudicate complaints concerning the activities of royal officials. The grievances that made their way into the *enquêtes* reveal a kingdom sorely tried by the centralizing policies of Philip Augustus (for a surprisingly large portion, if not, indeed, the majority, of the grievances recorded by the *enquêteurs* address confiscatory acts carried out by royal *baillis* under Philip) and provide a striking

illustration of how far royal power had penetrated into all parts of the realm. What is equally interesting is that the *enquêtes* demonstrate that the counselors of Philip Augustus had already abandoned a feudal model of royal authority, which Philip himself had used to such effect, in favor of a new "seigneurial" view of the realm, in which a *seigneur-roi* ruled over a vast domain-like "reserve," whose inhabitants, both ecclesiastical and lay, were construed to be his "tenants." It was this "seigneurialisation" of the conception of the kingdom that made it possible for the king to demand a host of services and dues that hitherto had escaped royal control, not least among them taxes and other monetary contributions from the rich towns of the northern reaches of the realm. This view of the realm as a "seigneurie," divided between royal "reserve" and tenants, made it unnecessary for Louis IX to continue to augment the royal domain, the enlargement of which had been a relentlessly pursued Capetian policy from the time of Philip I onward, since those areas that did not fall within the "reserve" could be considered to constitute "tenancies" and were thus subject to royal exactions. Sivéry argues convincingly that the conceptualization of the kingdom as a "seigneurie" was an important transitional phase from a feudal to a juridical model of the state, which, with the aid of Roman law, was to triumph by the end of Louis's reign.

The remainder of the book is devoted to a precise exposition of the new administrative techniques and personnel developed under Louis to exploit the resources of his extensive kingdom and to the political resistance that royal action engendered on the part of his subjects, especially the nobility, who suffered most from the revival of royal authority. It is the great merit of this work that it does not present royal progress as an unmitigated blessing. Sivéry is equally sensitive to the tensions, as well as to the undoubted benefits, that the enhancement of royal power created—tensions felt on every level of French society, which the king's own devotion to justice and indisputable sanctity did much to mask and mollify.

If I have any complaint with the book, it is that the chapters on the cultural movements of the *siècle* of St. Louis add little that is new and only serve to enlarge an already massive work. For historians interested in the administrative history of the Capetian monarchy, however, it offers an invaluable and fascinating view of a critical moment in the development of the French state.

GABRIELLE M. SPIEGEL
University of Maryland

C. T. ALLMAND. *Lancastrian Normandy, 1415–1450: The History of a Medieval Occupation*. New York:

Clarendon Press of Oxford University Press. 1983. Pp. xiii, 349. \$47.50.

In his preface to this book, C. T. Allmand announces two objectives: first, to achieve a synthesis of previous scholarship devoted to aspects of the English conquest and occupation of Normandy in the fifteenth century; and second, to present new material on a number of topics, such as land settlement, public opinion, and the tension between Paris and Rouen. He has accomplished both goals successfully, producing the authoritative work on Lancastrian England as an occupying power.

Allmand devotes two chapters to the Anglo-French fighting and two more to English settlement in Normandy, with particular reference to the city of Caen. Another chapter explores relations between the *Cour du Conseil* at Rouen and the parliament at Paris. Allmand also discusses military organization, diplomacy, economic and social conditions, and public opinion on both sides of the channel. He concludes with a look at Normandy after the French reconquest and the celebrations of this "liberation" in more modern times.

Allmand tends to minimize the importance of the battle of Agincourt in 1415 and the Franco-Burgundian treaty of 1435. Instead he emphasizes the English capture of Harfleur in 1415, the murder of John of Burgundy in 1419, and the battle of Verneuil in 1424. (Harfleur was the key to Henry V's conquest of Normandy; the assassination enabled him to pursue the throne of France; and Verneuil guaranteed English possession of Normandy for another quarter of a century.) Loss of the fragile Burgundian alliance in 1435 was not a real turning point, but in the 1420s that alliance was important not only militarily but also in influencing important Frenchmen to enter English service. These conclusions will not persuade everybody. The deaths of many important Norman lords at Agincourt must have affected the capacity of their lands to resist Henry V's conquest. The end of hostilities between France and Burgundy surely facilitated Richemont's capture of Paris in 1436, an event of great importance for the French.

The most valuable new material in this book concerns the extensive English settlement in Normandy and the careers of these who spent most of their lives living and serving in Normandy. Allmand has drawn heavily on archival materials in reconstructing the process that made the Lancastrian war truly different from other phases of the long Anglo-French struggle. Henry V revived and strengthened the tradition of an independent Normandy tied to England rather than to Paris. These efforts became an embarrassment when the events of 1419–20 gave Henry the expectation of the French throne, but after Verneuil the English accelerated their settle-

ment of Normandy and Bedford continued to strengthen the *Cour du Conseil* at Rouen. After 1436 the settlement of Normandy limited English diplomatic options and forced them to fight a defensive war on French soil.

The English Parliament was far more ready to vote funds to defend Calais than to support the war effort in Normandy. The Norman Estates bore much of this responsibility at a time of serious economic and social trouble. Conditions seem to have been somewhat less severe in Lower Normandy, where Caen was a site of major English settlement and the founding of a new university.

For most of the Norman population the English occupation was a matter for passive acquiescence or passive resistance. The higher clergy were most willing to accept English rule. Following the French reconquest, the settlement of conflicting property rights was a difficult and contentious business, but Allmand credits Charles VII with managing it rather well.

This is a thoroughly solid piece of scholarship, with an excellent bibliography and useful footnotes that are where they belong and not stuffed uselessly at the end of the book.

JOHN BELL HENNEMAN
Princeton University Library

MODERN EUROPE

NORMAN L. JONES. *Faith by Statute: Parliament and the Settlement of Religion, 1559*. (Royal Historical Society Studies in History Series, number 32.) New Jersey: Humanities Press or Royal Historical Society, London. 1982. Pp. viii, 245.

In a book likely to be read only by scholars already acquainted with the sources, Norman L. Jones has chosen to challenge an accepted interpretation. By a close examination of familiar materials, the author tries to show that Sir John Neale's analysis of the Elizabethan religious settlement of 1559 was "too simplistic" (p. 4). Jones agrees with recent work by Winthrop S. Hudson that the main opposition to the original proposals for an English church came not from Neale's Puritan party, but from Catholics in the House of Lords. Particular focus is on the key role of the Marian bishops in forcing the queen to pursue a new course after the Easter recess of the 1559 Parliament. The author also gives careful scrutiny to the European background of events taking place in Westminster.

The most interesting portion of the study is a precise examination of the membership of both the Lords and the Commons, revealing that the Puritan core was not nearly as large as supposed. Jones

concurs with Hudson's assessment that the role of the Marian exiles has been much exaggerated both as to numbers and cohesiveness of viewpoint. Jones feels the focus should be on the House of Lords where the Catholics were both numerous and powerful enough to compel the queen to take measures to reduce their influence during the Easter break. Rather than, as Neale argued, Elizabeth being forced to compromise with militant Protestants, she was the one who had to "save the day for the reformed religion" by vigorous action (p. 103).

Jones provides a comprehensive case for his view that may, in time, become the accepted interpretation. But his monograph is not one likely to attract a wide readership to its minute account. As Patrick Collinson has pointed out, Neale's truly original contribution was to show the interplay of political and theological factors, even if his exact conclusions need to be revised.

One additional point that *Faith by Statute* reveals is just how useful a modern, detailed biography of Nicholas Heath, Archbishop of York and leader of the Catholic forces in 1559, would be to the student of any aspect of the transition from Marian to Elizabethan England.

JOEL BERLATSKY
Wilkes College

J. I. KERMODE and C. B. PHILLIPS, editors. *Seventeenth-Century Lancashire: Essays Presented to J. J. Bagley*. (Transactions of the Historic Society of Lancashire and Cheshire, number 132.) Gloucester: Alan Sutton, for The Society. 1983. Pp. 194.

The seven essays honoring J. J. Bagley maintain the Historic Society's long tradition of scholarly excellence in regional history. Each explores a significant question related to seventeenth-century Lancashire and offers new insights rooted in a balanced examination of appropriate county records and the most recent scholarship.

R. G. Dottie reappraises the recusant riots at Childwall, arguing that pressures from Puritans, the Privy Council, and churchmen, exacerbated by a zealous constable, turned tension into riot in 1600. Only the overall sensitivity of public officials localized and defused the situation. Next, T. S. Willan examines the effects of mortality on economic life and social structure following the 1605 plague in Manchester. Despite a rapidly rising death rate, the author found slight impact on the town's administration or land ownership. In the third essay D. J. Wilkinson studies the structure of the commission of the peace before 1642, demonstrates its stability and continuity, and concludes that it illustrates Lancashire's growing assimilation into the English mainstream. B. W. Quintrell sees the investigation of

Bishop John Bridgeman's diocesan administration in 1633 as the most vigorous incursion into the region during Charles I's reign. Bridgeman's nig-gardly response to William Laud's appeal for the repair of St. Paul's probably prompted the inquiry. Allegiances and issues in Civil War Lancashire attract B. G. Blackwood's attention. He finds religious differences more divisive than social or local ones, though he notes stronger parliamentary support among the common people.

The last two essays concern the social and political position of the earls of Derby and the Restoration militia respectively. B. Coward argues that the seventh earl was not a fanatical royalist; rather, he was a cautious man who sought to maintain then restore his family's dominant local authority. D. P. Carter documents the militia's continued survival after 1660 and suggests its broad importance.

Never dull, this slim volume blends new and traditional methods to introduce recent trends in seventeenth-century Lancashire studies.

MICHAEL J. GALGANO
James Madison University

R. W. HARRIS. *Clarendon and the English Revolution*. Stanford: Stanford University Press. 1983. Pp. 456. \$39.50.

Edward Hyde, earl of Clarendon, is one of the most interesting and important figures in the history of seventeenth-century England. A member of the Great Tew circle (where he learned to admire the principles of the Church of England) and a successful common lawyer with a devotion to the principles of that law, he began his political career as an opponent of the regime of Charles I because of what he regarded as the excesses of the king's government. By the latter half of 1641, however, he was becoming alarmed at the excesses of King Pym, who, says R. W. Harris, "had the clear objective of dismantling the king's government" (p. 65). Hyde's concern for the law and the church brought him over to Charles's side, where he quickly became one of the embattled monarch's principal advisors. In March 1645, Charles appointed him to the council of the Prince of Wales, and, from that time forward, Hyde served that ungrateful young man both in exile and after the happy restoration of 1660, until his dismissal as Lord Chancellor in 1667. From 1660 to 1667 he was, after the king, the most important man in the English government. Once again in exile after 1667, he finished his great history of the rebellion, which he had begun in the late 1640s, and, like so many other fallen statesmen, he added a continuation justifying his conduct in office.

A reassessment of the career of this remarkable man is long overdue. There is no modern biogra-

phy, partly because the sources for his life are scarce apart from his own account, and the one recent book devoted to him, Brian Wormald's *Clarendon: Politics, History, and Religion 1640–1660* (1951), is an account of his intellectual evolution rather than his political career. It is disappointing to have to report that this book does not fill the void. What Harris has attempted to do is to write an account of Clarendon's public life as a kind of commentary on his *History*; the result is neither a biography nor a full-scale history of the period, but a rather unsatisfactory combination of the two. Clarendon is a hero to him, "the greatest royalist statesman of the seventeenth century and the first of the great English historians," as we learn in the opening sentence of chapter one. This point of view has its value as a corrective—most scholars do not stress, as Harris does, John Pym's frequently unscrupulous and underhanded behavior. But the narrative, as a whole, is lacking in conceptual analysis, a particularly regrettable shortcoming in the chapters on Clarendon's years of power during the Restoration. Furthermore, the author's historical knowledge seems rather limited. That he frequently errs in his account of Scottish affairs is not, perhaps, so surprising; most historians of England do. But it is not easy to see why he finds James I's hostility to the views of Conrad Vorstius difficult to understand (p. 15) since Vorstius questioned the eternity and omnipotence of God; nor is it reasonable to suppose that William Laud's lost letters to his godson William Chillingworth would, if found, reveal the archbishop as a tolerant and liberal man (p. 20). Toward the end of the book the writing becomes slapdash: three times in the last eighty pages (pp. 339–40, 341; 382, 387–88; 407, 413–14) the author repeats himself almost *verbatim*. It is as though neither he nor his editors, reaching the end of their long and weary road, took the trouble to read over what he had written. This is not a bad book, but it is dull and it breaks no new ground.

MAURICE LEE, JR.
Rutgers University

LOUISE LIPPINCOTT, *Selling Art in Georgian London: The Rise of Arthur Pond*. (Studies in British Art.) New Haven: Yale University Press, for the Paul Mellon Centre for Studies in British Art. 1983. Pp. xii, 212. \$27.50.

The historical sociology and economics of British art is a regrettably neglected field of research. Neither art historians nor historians of culture have been inclined to probe into the manifold relations between art and the society that supported it, not least in the age of Hogarth and Reynolds, when paintings, prints, and sculpture became commodities

sought by an increasingly affluent segment of the educated elite. Part of this neglect has been due to the lack of easy access to particularized sources. Apart from such primary works as George Vertue's *Note Books* and Horace Walpole's *Anecdotes of Painting*, there has not been much for persons curious about the subject to sink their teeth into without going to a great deal of trouble.

Louise Lippincott has gone to the trouble, and with the publication of her careful, fact-laden monograph on Arthur Pond—prolific but uninspired painter, engraver, and dealer—the prospect of a better-grounded understanding of the art trade in eighteenth-century London brightens. The book's nucleus is a collection of Pond's business papers, covering the years 1734–50, which had lain untouched in the British Museum's Department of Manuscripts for a century. Any such treasure trove of artist's papers is valuable, but Pond's has special usefulness because, as Lippincott says, he is distinguished by "his very lack of artistic importance; examination of his life and work usually reveals the typical rather than the exceptional. The known facts about him match and illuminate the surviving fragments of information remaining for his contemporaries" (p. 160). Lippincott does not study Pond's records in isolation; she amplifies and substantiates their typicality by creating a context from documents at the Public Record Office, the Guildhall and Bodleian libraries, memoirs of Pond's contemporaries, auction records, and catalogues of prints and drawings.

The result is an informative account of the commerce in art, and the artist and dealer as entrepreneur, in mid-century Britain: the machinery of patronage, the merchandising of art objects, and the artist's place vis-à-vis other purveyors of art and culture to the aristocracy and upper bourgeoisie. The data Lippincott has collected are extensive and detailed enough to enable her even to construct tables showing the social rank and buying habits of patrons, the prices paid for portraits as *objets de luxe*, the distribution of painting costs (expenditures for pigments, crayon papers, brushes, stretchers, frames, etc.), and the distribution of art-related income among painters, dealers, printsellers, framers, and teachers. This is the nitty-gritty of art history, leagues removed from the world of Hogarth's *Analysis of Beauty* and Reynolds's *Discourses*, but just as essential to our comprehension of the actual atmosphere of the art world in their time. Pounds, shillings, and pence figured as prominently in artists' lives—and their ambitions—as aesthetic theory, and making a comfortable living (and, to some, achieving a social status generally denied members of a craft) was as desirable as storming the heights of the Grand Style.

"If the visual arts are subjected to the same kind

of inquiry scholars have directed at literature (for example), perhaps the apparent differences between the art world and other cultural spheres can be resolved, and painters and their patrons will join the writers and readers, the actors, dramatists, and theatre-goers of the eighteenth century" (p. 6). Lippincott's ground-breaking study encourages the hope that other historians will follow her in helping the thought become father to the deed.

RICHARD D. ALTICK
Worthington, Ohio

ROBERT R. DOZIER. *For King, Constitution, and Country: The English Loyalists and the French Revolution*. Lexington: University Press of Kentucky. 1983. Pp. xi, 213. \$22.00.

Among the popular movements of the late eighteenth century, the loyal associations of 1792–94 were the most successful. Robert M. Dozier's study traces their origins and ultimate triumph in the early years of the decade. Indeed, his thesis is that they were so successful in their activities that their reason for existence disappeared after the danger of French invasion and subversion declined in the summer of 1794.

The English loyalists emerged as an organized force in politics in the early summer of 1792. Their organizations, which conformed largely to the models of public political societies in the last half of the century, arose spontaneously in response to the Proclamation of May 1792. In that document the king had warned of seditious and other "wicked" writing. The intensity and the extent of the response, largely in the form of loyal addresses, surprised the reformers and the ministers alike.

The second outpouring of loyalist activity took place in late 1792 and early 1793. At this time the uncoordinated and hitherto largely dissimilar local groups formed into associations pledged to support the constitution and British institutions. This activity was clearly fostered and financed in part by Pitt's ministry, as the author has shown in his discussion of the role of John Reeves in the formation of the Association for the Preservation of Liberty and Property against Republicans and Levellers.

The final stage came in 1794 and was stimulated by the prospect of a French invasion and the revival of the radical organizations of Hardy, Tooke, and others. At this time loyalists and patriots (the two were not always the same; the latter were interested principally in national defense, not in constitutional ideology) came together to form militia and cavalry units for the defense of local areas.

The importance of this book is the clarity with which the story of the supporters of the English system is told. The emergence of loyalist sympathies

may have been stimulated by the threat of French invasion and the fear of change if it entailed, as it seemed it might, the unleashing of uncontrollable political passions and social and constitutional reforms. But their activities were sustained clearly by a positive response to the best of English traditions. English nationalism was thus embodied in loyalist activities and ideas. This gave them the upper hand in conflicts with the more internationalist reformers. Since the latter have long been well-served by historians, it is time that the loyalists had their historian as well. In this account they have found him.

CHARLES R. MIDDLETON
University of Colorado

ROGER MORRISS. *The Royal Dockyards during the Revolutionary and Napoleonic Wars*. Leicester: Leicester University Press. 1983. Pp. ix, 262. £28.50.

This is an important book for two reasons. First, it offers a comprehensive account of the situation and growth of Britain's largest industrial organization during a stressful period. Second, its leitmotiv is the phenomenon of reform. This review will focus on reform because the theme concerns all historians of the period.

Public disappointment in two wars put dockyard reform on the political agenda. Partly because the Admiralty had already instituted an innovative task-work system in 1775, the dockyards were not prime objects of the unprecedented public clamor for governmental efficiency that suddenly sprang up during the American Revolutionary War. Charles Middleton, Comptroller of the Navy, tried to institute certain dockyard reforms in the later 1780s, but there was not enough public pressure or cabinet support to overcome conservatism and entrenched interests. When the next war also began to go badly (in the mid-1790s), however, some of his lingering proposals were quickly implemented. The pressure for improvement did not relent. After 1800, except for one year of peace, the dockyards were expected to fight a major war and change their ways at the same time (pp. 192–93). As it happened, some crudely conceived measures implemented by Lord St. Vincent actually inhibited expansion of naval force in 1803–05; fortunately, the skill and heroism of the sea service—of which Lord Nelson's name is rightly emblematic—prevented serious consequences. After 1805, following a change of regime, reform followed more constructive channels and the results were impressive. By 1815 the royal navy's shore establishment attained a level of quality worthy of the renowned sea service. This high level stemmed partly from new facilities but mainly from technological and administrative innovation.

Naturally, facilities and technological innovations

were the most visible, but the key to improvement was administrative. The central achievement was a wide-ranging purification of the structure of appointments, promotions, and work incentives. From top to bottom the old structure of personal connection and dependence was supplanted by bureaucratic professionalism. As Roger Morriss summarizes on page 156: "These changes can be properly seen as preparing the way for the great developments in the civil service in the mid-nineteenth century. Through the abolition of fees, premiums and gratuities, the improvement of salaries and extension of superannuation schemes, officials were made to a large extent independent of pecuniary influence from those with whom they dealt—whether contractors or subordinates—and completely dependent on the 'one certain salary' and pension paid by government." It is fascinating to observe how many consequent reforms this process spawned; the reorganization of shipwrights' apprenticeship and its social implications is an interesting example. Perhaps the most impressive quality of the book is its skillful display of the interconnectedness of administrative reform.

On one general point the author's interpretation may be questionable. It concerns the principle of "individual responsibility," as opposed to collective or corporate responsibility. "Individual responsibility" was pressed forward with ideological fervor by Samuel Bentham, inspector general of naval works. Collective responsibility was traditional: it was enjoined on officials since the seventeenth century, if not earlier, by rules that routinely required them to cross-check each other's business. "Individual responsibility" is portrayed in the book as representing the wave of the future; collective responsibility the habit of the past.

There is something in this, but the emphasis is overdone. It gives the individualist principle too much importance, and Samuel Bentham too much credit for originality and sensible diagnosis. It understates the degree to which yard operations had always been under the individual direction of the master shipwright (or master attendant in his special sphere) and demotes the fact that Navy Board members had always had individual as well as collective duties. It does not fully confront the problem of audit control (the prime reason for the old "check system"), a problem that could not—and cannot—be adequately handled through "individual responsibility." In fact, the elimination of the check system stemmed from more effective means of accounting, new administrative attitudes that shifted the focus from accountability toward operational effectiveness, and an inclination—common in the reform era in Britain—to bring rules into line with approvable existing practices. (During the eighteenth century the dockyard officers had seldom

executed their cross-checking duties in person.) Perhaps it may be contended that "individual responsibility," though not the motivating force of reform, played a vital role in shaping its style and direction. The trouble with this view is that it fails to consider the principle's inappropriateness to a service industry whose officers were not ordinarily subject to dismissal and whose efficiency depended heavily on cooperative and collective as well as individual efforts. The argument could be carried further, but this is not the proper forum. Enough has been said, however, to indicate that when investigating reform in the royal dockyards in 1800 one surprisingly finds oneself at the core of the controversy over the "nineteenth-century revolution in government"—not least because Samuel Bentham was Jeremy's older brother.

All in all, the book provides not only a rich and suggestive history of reform but also a descriptive account of the dockyards and their organization. The task of research was formidable: Morriss had to examine both the official records and the many volumes of parliamentary reports of inquiry. The work has been done with care and analytical skill, and is therefore a major contribution.

DANIEL A. BAUGH
Cornell University

DEBORAH GORHAM. *The Victorian Girl and the Feminine Ideal*. Bloomington: Indiana University Press. 1982. Pp. 223. \$20.00.

In *The Victorian Girl and the Feminine Ideal*, Deborah Gorham analyzes how nineteenth-century middle-class English parents raised their daughters to become proper Victorian women. The book is divided into three parts: the first discusses ideas about girlhood in the larger context of Victorian views of womanhood and femininity; the second, how these ideas were translated into concrete advice about rearing daughters; the third, the actual lives of fourteen nineteenth-century girls.

Gorham's discussion of Victorian ideas about women and girls is a valuable addition to the important work done on this subject in recent years. Her emphasis on girlhood switches attention to a point in the female life cycle frequently slighted in favor of concentration on woman's role as wife and mother. In fact, as Gorham points out, the Victorian girl embodied contemporary ideals of femininity far better than her mother. She could appear to be unambiguously dependent, childlike, and sexually pure, while her mother was caught between the ideal of asexuality, dependence, and gentility on one hand, and the reality of her active sexuality and hard work and competence in the home on the other.

Throughout, Gorham emphasizes that there was

no such thing as a "Victorian ideal of womanhood," but rather a set of ideas that evolved and changed throughout the nineteenth century. Thus, she discusses, quite convincingly, early, middle, and late Victorian girlhoods. She also points out that there was no such thing as a middle-class life style. Only a small fraction of those who considered themselves middle class could actually afford the ideal pattern of life, which required a home large enough to accommodate a separate nursery wing and a complement of three or more servants. Furthermore, whatever the family's standard of living, it was a precarious one from the daughters' point of view. Their educations and expectations of economic dependence hardly prepared them for the financial crises that followed their fathers' deaths or failures in business—a common experience in nineteenth-century England if the biographies in this volume are at all typical.

The most compelling part of Gorham's book is the concluding section, which covers the lives of fourteen Victorian girls. The author has grouped them chronologically: five were born between 1819 and 1834, six in the middle decades of the century, and three in the 1880s. Within each group, she carefully locates individual families within the middle class and explores the impact on their daughters of such factors as religion, the number of children in the family, their daughters' place in the birth order, and whether the families were upwardly or downwardly mobile. She also takes account of the unique personalities of the girls and their families and any crucial relationships or opportunities that developed outside the nuclear unit.

The result is a rich mosaic that once again underscores how difficult it is to speak of a "typical" Victorian family once one begins to look at specific examples. The one general point that comes through over and over again is how crippling and tragic inadequate education and economic dependence were for nineteenth-century middle-class women. The opening of educational opportunities from the 1860s onward was a real landmark in the history of English women. By the 1890s and early twentieth century even work outside the home was becoming respectable. If Gorham's book has any heroes, they are certainly Frances Buss, Dorothea Beale, and the other women and men who fought to open secondary schools and colleges for women. For American feminists with their eye on fundamental social change, who all too often classify the struggle for education as a "conservative" reform, it is chastening to be reminded of what life was like for a middle-class girl growing up in London before the opening of the Cheltenham Ladies' College, North London Collegiate School, or Bedford College.

BARBARA J. HARRIS
Pace University

ROBERT J. WALLER. *The Dukeries Transformed: The Social and Political Development of a Twentieth Century Coalfield*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1983. Pp. 319. \$45.00.

In Nottinghamshire, coal has been worked from Roman times on the exposed coalfield (where the seams outcrop) down the west side of the county, and this area is called the Erewash Valley. In the second half of the nineteenth century new pits were sunk further to the east in the Leen Valley, also known as the old concealed coalfield. During the present century this process has been carried a stage further by the sinking of mines still further east, in the area known as the Dukeries—the new concealed coalfield.

Many of the mining folk who immigrated into the Dukeries shortly after the pits were sunk are still alive, and their oral evidence is vividly presented in this excellent study. The colliery proprietors built their new, isolated mining villages around the pit heads in what had been a rural environment. The villages were "company towns" with the houses and many other buildings owned by the colliery companies. Social control was exercised by an oppressive paternalism so that villages were kept neat and tidy, unruly behaviour was rare, and political organization among the mining families was virtually nonexistent. A split in the Nottinghamshire Miners' Association during the long 1926 dispute led to the formation of a "breakaway" union—the Nottinghamshire and District Miners' Industrial Union, popularly known as the Spencer Union after its leader George Alfred Spencer. This body, which the owners fostered, guaranteed them immunity from strikes in return for sole negotiating rights.

There was, inevitably, tension between mining folk and the indigenous population. Also, the isolation of new villages, delays in providing schools and services of various kinds, and the bringing together of people from different parts of the country produced tensions that stimulated a high rate of labor turnover, and this in its turn contributed to the generally unsettled state of these new communities.

This is the best study of its kind that I have read, but it has its weaknesses. First, no mention is made of the task required of men in these new pits. On average, a collier got and loaded onto a conveyor belt twenty-seven tons per day by hand. What effect did sheer exhaustion have on the collier's way of life? Is there a causal connection between the heroic task and the "butty" (subcontract) system?

Second, although Robert J. Waller introduces studies of other new industrial communities of various dates, he makes no comparison with the Leen Valley development of the late nineteenth century in which the philanthropic Quaker Liberal

M. P. John Edward Ellis played such a leading part. Similarly, he does not contrast the benevolent paternalism, which was so evident among aristocratic and liberal nonconformist colliery owners in the Erewash and Leen valleys, with the pseudo-benevolence of the companies that exploited the Dukeries.

Even more surprisingly, there is no contrast with the two Dukeries mines (Mansfield and Welbeck) sunk well before the Spencer Union was formed, nor with Bevercotes, sunk after nationalization, which is the only mine in the Dukeries that does not have a pit village.

ALAN R. GRIFFIN
University of Nottingham

ELISABETH BARKER. *The British Between the Superpowers, 1945–1950*. Buffalo: University of Toronto Press. 1983. Pp. xii, 269. \$20.00.

The continuity of Britain's foreign policy is a tradition sometimes assailed in Britain and often presented by critics as the ideal in American foreign policy formulation. Yet, if one shares an aversion to proclamations of mint-new policy courses and to disregarding commitments, the key question one might ask is: What in British policy should be continuous?

In the concluding chapter of her book, Elisabeth Barker affirms a continuity of "dangers, problems and painful dilemmas" that pulled "Britain first one way, then that, in the 1950s, 1960s, 1970s and after." Relations with the Russians "continued to swing" between deep mistrust and star-crossed efforts toward understanding; relations with the Americans "went on oscillating" between the pursuit of close association and "the desire for independence." Her story, she concludes, "has no end and its moral is debatable" (p. 231).

Barker's pages convey the experience of postwar policy making and maintenance as an ordeal involving many more essential interests than just the support of power: they tell of impossible hopes—a story that cannot have bold lines. She has written history somewhat to the design of Samuel Rawson Gardiner, that is, blotting out any knowledge of subsequent events in composing each section of the history. She has used Cabinet, Foreign Office, and Defence (but apparently not Colonial Office) documents, and Labour party archives. She has performed the difficult feat of achieving a clarity and liveliness in writing that is close to the order and tone of her sources.

The brief first chapter on the war years notes that British wartime censorship fostered optimistic popular expectations of Soviet friendship and cooperation to which the left wing of the Labour party appealed. Official propagandists and the Political Warfare Executive (PWE) "lived mostly in this semi-

mythical world" (p. 2). For example, the PWE's hopes were at variance with the work of the Post-Hostilities Committee (P.H.C.), which, in 1944, were considering, in traditional fashion, the likelihood of Russia being tempted to fill the vacuum that went with European weakness. In the worst possible case—Russian hostility—the PHC concluded that only Germany could, with its geographical position, manpower, and resources provide the aid essential to Britain's preservation.

The book concludes with the close of 1950, a year chosen not because it is a climax or turning point but because a series of interrelated problems then reached a stage of definition and compelled choices. In the effort to deal with persisting dilemmas, human energies were lavishly and cruelly expended: Foreign Secretary Ernest Bevin and Chancellor Sir Stafford Cripps gave up their posts because of illness and soon died. They and their colleagues had achieved more than their country's actual strength warranted.

The final chapter presents the author's deeply felt, well argued, and judicious conclusions. Barker has forestalled the criticism that she gives only the bare essentials of atomic issues by pleading that she did not want to do again Margaret Gowing's *Independence and Deterrence*. She also claims that her understandable decision not to take on the task of using American documents, published and unpublished, means that her story is not complete. The book should be the starting point of any further work on the period.

M. A. FITZSIMONS
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R. D. ANDERSON. *Education and Opportunity in Victorian Scotland: Schools and Universities*. New York: Clarendon Press of Oxford University Press. 1983. Pp. vi, 384. \$59.00.

R. D. Anderson's *Education and Opportunity in Victorian Scotland* is an important study that revises several long-accepted generalizations about Scottish education and social mobility. Anderson attacks the view, argued most influentially in G. E. Davie's *The Democratic Intellect* (1961), that Scotland possessed an open educational system that made university entrance possible for the "lad of parts," regardless of class background. Furthermore, this interpretation holds that the traditional Scottish system was destroyed in the nineteenth century by the proponents of "Anglicization" who wished to imitate England's narrower university curriculum and more stratified educational system. Although Anderson recognizes this view as a potent "historical myth," which had a significant role in shaping educational policy, he also

argues that it is smug, simplistic, and often factually incorrect.

Anderson is especially critical of the romantic glorification of the "Scottish tradition." He argues that secondary education was poorly developed and that the integrity of the broad, philosophical curriculum of the universities was often undermined in practice by low academic standards, casual attendance, and low percentages of graduation. Scottish education might seem "democratic" when compared to the extreme stratification characteristic of nineteenth-century England, but it appears much less remarkable when viewed in a broader European context.

The greatest contribution of Anderson's study lies in his careful testing of accepted generalizations through statistical analysis. He finds that the parish schools, so often glorified for their mixing of the social classes and for their effectiveness in making university entrance possible for poor youths of ability, were actually of greatest service to the rural middle classes—the sons of ministers and schoolmasters themselves. The great majority of genuinely poor students who reached the universities were older men who had worked after leaving school and had prepared themselves independently for university entrance. The mixing of classes has also been much exaggerated: Anderson finds some supporting evidence from rural schools, but the burgh schools of the cities were already highly stratified in the early nineteenth century. In fact, Anderson argues that there was actually more social mobility through education at the end of the nineteenth century than there had been at the beginning. The development of secondary education and the provision of bursaries to allow talented elementary students to transfer to secondary schools, proved more serviceable to the urban working class (though more to the small shopkeeper class and to skilled artisans than to common laborers) than the traditional system of direct access from the parish school to the university had been. Nonetheless, Anderson concludes that the degree of actual mobility was very limited and that it has only been the persistence of an exclusively English framework of reference that has permitted the perpetuation of the "democratic" myth.

ARTHUR ENGEL

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JEAN GALLET. *La seigneurie Bretonne, 1450–1680: L'exemple du Vannetais*. Foreword by ROLAND MOUSNIER. (Travaux du Centre de Recherches sur la Civilisation de l'Europe Moderne, number 22; Histoire Moderne, number 17.) Paris: Publications de la Sorbonne. 1983. Pp. 647. 180 fr.

Jean Gallet's study analyzes the seigneurial system in some sixty parishes around the city of Vannes, in lower Brittany. The work proceeds systematically, in the three-part structure of classical French scholarship. A first section describes the seigneurial system in about 1480, before Brittany's union with France and before the dramatic economic changes of the sixteenth century. Even in the late fifteenth century, Gallet suggests, both the seigneurie and the province's nobility retained an essentially medieval character. Part 2 traces the changes that transformed the seigneurie over the sixteenth and seventeenth centuries. Part 3 analyzes the seigneurial system in about 1680.

This is a *thèse d'état*, and many of its qualities are those of the genre. Gallet presents an exhaustive analysis of a massive range of documents, but he makes few concessions to the nonspecialist reader; he enters rarely and with extreme caution into scholarly debate; and it is the reader who must usually supply the scholarly context from which the book's significance emerges.

Yet an important theme unites the mass of detail that Gallet presents. That is the decline of the lordship, a decline most marked in the case of the large lordships that had dominated the region in 1480. Their demesnes shriveled, their revenues failed to keep pace with inflation, and their public powers decayed. Decline of the large lordship was matched by the rise of smaller estates that lacked the public powers of lordship and derived little revenue from seigneurial dues. The rise of these estates came at the expense of the large lordships, and at the expense of the peasantry as well. Peasant property virtually disappeared from the region in these centuries.

Gallet thus presents a familiar pattern. He sees an upper aristocracy in crisis, forced by its dispendiousness and its lack of economic rationality to sell or enfeoff its estates; and he sees a corresponding rise of a gentry, here dominated by the magistrates of the parlement of Rennes. As an order the nobility remained firmly in control of the region's property, but the distribution of property within the order had changed and so had the nature of large property itself.

The study has a second principal concern—to understand the seigneurie's impact on the society around it. Here Gallet is more tentative, but he offers a similarly significant interpretation. The lordship, he suggests, had provided an important social framework in the fifteenth century, but by 1680 it barely touched most peasants' lives. Only its judicial functions retained some vitality, and these mainly in the limited sphere of property transfers.

The book has limitations. Gallet focuses more on property and institutions than on people. Thus we do not gain much sense of the pressures and values

that shaped nobles' economic choices. Gallet's admirable caution, his respect for the documents and for what he calls "the extraordinary variety of situation" (p. 569) create their own frustrations. He is reluctant to generalize about what was typical within this range of situations—whether about such straightforward matters as incomes or about more delicate issues such as the lordship's oppressiveness.

This is, nonetheless, important research on important questions. Its usefulness is magnified by recent debate about the evolution of property and the role of the nobility in early modern France. Readers will find scant reference to these discussions in Gallet's book, but they will find in it an absolutely solid contribution to our understanding of these issues.

JONATHAN DEWALD
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SARAH HANLEY. *The Lit de Justice of the Kings of France: Constitutional Ideology in Legend, Ritual, and Discourse*. (Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, number 65.) Princeton: Princeton University Press. 1983. Pp. xiii, 388. \$45.00.

There are hints of "more than you ever wanted to know." Repetition threatens to become bothersome. Yet this book works as a careful account of the development of the *lit de justice* assembly, especially during the sixteenth century.

The *lit de justice* originally (1387–1413) was the name given to the "draped apparatus which defined royal space" (p. 47) when the king held a royal *séance* in Parlement. From 1527 onward the name became attached to a particular type of royal meeting with Parlement, one concerned with constitutional matters. Antiquarians and historians, particularly Jean Du Tillet, sought the origins of the institution. Their work coincided with the efforts of royalists to bolster French attempts to meet the threat posed by the Habsburgs.

According to the criteria established by Sarah Hanley, there were no *lits de justice* between 1537 and 1563. There were four meetings during the Wars of Religion and one in 1597. The most crucial meeting after that of 1527 was the inaugural *lit de justice* of Louis XIII in 1610, which effected a change from the sixteenth-century theory "that the royal *dignité* was conferred upon the hereditary successor by virtue of French Public Law and that the act was recognized ceremonially in the traditional inaugural program (Royal Funeral, Coronation, and Majority)." Instead it became accepted "that royalty was conferred through [Public] Law and

Nature and was confirmed ceremonially by the new two-stage inauguration, *Lit de Justice* and Coronation" (p. 261).

From 1610 to the majority of Louis XIV there was a revival of the historical study of the *lit de justice* assembly that encouraged its use by royal officials to increase royal power. After the early 1650s, as emphasis was shifted from the concept of the development of the monarchy through a historical process to the concept of unchanging tradition, the *lit de justice* was much less important. Between 1614 and 1658 there were thirty-two meetings, six from 1662 to 1673, and then none until 1715.

Hanley cannot quite bring herself to give explicit credit to the regency government of Marie de Médicis for its careful engineering of the *lit de justice* of 1610. Yet she does not take the approach of Richard Bonney and some members of the Oxford School who feel obliged to insist that Marie de Médicis and her advisers, by definition, could not possibly have done anything worthwhile. Hanley also presents a clear picture of Parlement's actions in 1615—a rare feat.

One could quibble about some points. For example, the location of the *lit de justice* of 1610 is left a bit vague (compare pages 233 and 255). The book does not so much end as just stops. Yet it is a well-conceived and well-executed analysis of a difficult topic. The nature of the *ancien régime* is made clearer as a result of this book.

J. MICHAEL HAYDEN
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ELIZABETH WIRTH MARVICK. *The Young Richelieu: A Psychoanalytic Approach to Leadership*. Chicago: University of Chicago Press. 1983. Pp. x, 276. Cloth \$32.00, paper \$14.00.

As a partisan of psychohistory I wish I could report that Elizabeth Wirth Marvick's psychobiography of the young Richelieu is an unqualified success. For reasons that follow, I cannot.

In part 1, "Perspectives," the author claims that, because of his intense ambition, attitude toward rule, self-control, manipulative prowess, and passionate patriotism, Richelieu was part of a generation of "new men." Had Marvick examined more closely the careers of the previous generation of royal ministers she would have realized that, by exaggerating Richelieu's novelty, she has thrown her perspective out of focus. As the cover blurb notes, "his actual achievements have been considered by many to be no more important or enduring than those of his predecessors or successors."

Part 2, "Precedents," demonstrates plausibly how Richelieu's family constellation helped shape his personality. By identifying with and modeling him-

self after persons like his mother, his older brother Henri, and his uncle Amador de La Porte, the future statesman took on some of their traits. Unfortunately, Marvick does not clearly delineate the composite effect of these varied influences.

Part 3, "Conditions," is the only section of the work in which the author fulfills her promise to apply psychoanalytic theory, that is, to listen with "an ear that is attentive to latent meanings in imagery and emphasis, in oversight and inconsistency, in repetition and denial" (p. 23). Marvick searches attentively for unconscious sources of Richelieu's ambition and speculates on why he chose to acquit it through service to the monarchy. Richelieu's early experiences of enuresis and discipline by shaming were important but not exclusive formative influences. Following Otto Fenechel, Marvick believes that ambition was Richelieu's defense against shame. The adult Richelieu's fears of failure, sense of urgency, insomnia, and frequent resort to tears lend plausibility to these speculations.

The last part of the book, "Preparations," is a narrative account of Richelieu's first try for power, culminating in his capturing the attention of Marie de Medici by his activities in the Estates-General of 1614–15. Little is new here, and one would never guess, were it read separately, that this is part of a psychobiography.

Although Marvick's scholarship is admirable and her speculations individually persuasive, the work is marred, in my opinion, by two fundamental flaws. The first, a matter of execution, is that her arguments are only tenuously integrated, and the reader is left to assess for himself the importance of the various influences that shaped Richelieu's adult personality. Second, the book is flawed in its conception. By presenting the future minister's childhood and youth apart from his years of political power, Marvick has ignored the most important goal of psychobiography—to show how the formation and operation of an important individual's personality influences public policy, that is, history. If Richelieu the man bore within him the childhood scars of shame, for example, one wants to know how these unconscious memories shaped his conception and execution of royal policy. Without making such connections, the study of personality becomes a kind of psychohistorical antiquarianism. In a promised second volume focusing on Richelieu's and Louis XIII's collaboration, perhaps Marvick will address this central concern of psychobiography.

EDMUND H. DICKERMAN
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ALAIN COLLOMP. *La maison du père: Famille et village en Haute-Provence aux XVII^e et XVIII^e siècles*. Foreword by EMMANUEL LE ROY LADURIE. (Les Chemins de

l'Histoire.) Paris: Presses Universitaires de France. 1983. Pp. 340. 150 fr.

What social historian conscious of the wealth of marriage settlements, property transfers, and wills available in French notarial archives has not wished this detail could be woven into a unified picture of village relationships? What researcher into families and genealogies has not been frustrated by the narrow antiquarian focus of the amateur enthusiasts who are usually the only contributors to such an obscure subject? This unusual book by an "amateur" historian merges antiquarian curiosity with scholarly expertise to produce what is probably the most detailed study ever written of family relationships in a small, early modern community. Using all the notarial registers from his ancestral village, parish registers, church visitations, criminal archives, and some administrative sources, Alain Collomp has reconstructed a tiny universe out of the family alliances and succession strategies that prevailed for over two centuries in Saint-André-les-Alpes, a small village located in the hills of Haute-Provence.

The book is unorthodox in its presentation; Collomp's meticulous reconstruction of the patterns of biological and social reproduction is that of an ethnographer. Although affiliated with the *Annales* tradition, he does not follow the usual progression from geographical setting and long-term factors to culture and consciousness; in fact, the reader has to wait patiently even to learn the size of the village or its relationship to the outside world. Instead, Collomp plunges right into the intimate details of individual lives, family ties, houses, furniture, and hereditary occupations. A procession of names (including many Collomps, cited without comment) fills a succession of thumbnail sketches of household relations. The reading is hard going, reflecting the dry specificity of notarial contracts, but it is also rewarding. This is Le Roy Ladurie's *Montaillou* without benefit of the subjective words and thoughts of the inhabitants. There are interesting observations on seigneurial power and useful sections on migration patterns, festive life, and economic roles, but these are mere window dressing for the essential subject of kinship.

Here fascinating patterns emerge. This village, with its hundred or so houses crammed close together along five or six narrow streets, provided an unchanging number of dwellings into which 120 or so families had to fit themselves in a hereditary game of musical chairs lasting from the days of Henri IV to the French Revolution. These 120 families had only 30 surnames, 6 of which accounted for 86 households. The reason for this extreme endogamy was the tradition of marrying close to home—"in your street, in your family"—to keep the exchange of women and property within the clan.

Collomp reveals a powerful system of stem families in which the patriarch ruled the roost and designated a single male successor who moved into the house with his wife and children while the other sons and daughters were provided for elsewhere. These Mediterranean peasants established lineages in a manner usually attributed to the aristocracy! Such patterns can be traced in the living arrangements within the dwelling, in property settlements, in family nicknames that denoted common ancestors, and in the nature of family quarrels. They can be detected only over time—hence the immense value of Collomp's historical documentation (he speaks at one point of 500 marriage contracts).

This is not a "total" history of the village, nor does it discuss the implications for the larger field of family history—in fact, no mention is even made of similar studies outside of France. But within its narrow focus, the book yields magnificent results that all specialists in the field will want to examine.

WILLIAM BEIK
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RICHARD A. ETLIN. *The Architecture of Death: The Transformation of the Cemetery in Eighteenth-Century Paris*. Cambridge: MIT Press. 1984. Pp. xiv, 441. \$37.50.

Etienne-Louis Boullée's 1784 design for a grandiose cenotaph to Sir Isaac Newton was in the shape of a sphere, which was placed in the middle of a drum. Inside the sphere was an armillary sphere that, through ingenious lighting effects, would create the illusion of shining stars. The architect said of his creation: "The spectator would find himself transported into the sky as if by enchantment and carried on the clouds into the immensity of space" (p. 134). The monument was to have commemorated a great man of science and it was to have provided the observer with a transcendental experience—as if he were "annihilated by the extraordinary spectacle of an inconceivable space" (p. 136). The project was never built, but judging from figures in Boullée's drawings it was to have been on a colossal scale.

Almost exactly contemporary with this project was the Marquis de Girardin's Ile des Peupliers, located on his Ermenonville estate, which contained the tomb of J.-J. Rousseau. Here, too, the visitor was to be deeply moved by the surroundings, but cosmic awe was replaced by melancholy and nostalgia. Just as Rousseau tore away the hideous mask of death, a contemporary wrote, so was nothing to be somber or sad in the gentle, Arcadian landscaped park to which his remains had been consigned. The visitor, as he wandered about the grounds of Ermenonville, was to feel as if Rousseau were sleeping.

As different as these settings were, both represent

the new attitudes toward death that crystallized during the eighteenth century. The meaning and purpose of life underwent a profound change, and with that change came a different way of thinking about death and burying the deceased. In this superbly illustrated volume, one can see in architecture and cemeteries the new approaches to death.

Boullée's Newton cenotaph is one of many eighteenth-century projects that was to have commemorated the great. For many of the enlightened, fame became a substitute for heaven, and one way to celebrate immortal achievements was through monuments. Competitions were held for funerary monuments, and although few of the designs culminated in actual buildings, the drawings are a remarkable record of one vision of death. A year or so after Boullée designed his Newton cenotaph he planned an equally grandiose Turenne cenotaph in the shape of a pyramid. He also drew up plans for a rectangular cemetery with a pyramid in the center. By this time there was a national movement in France to reform burial practices. The dead, it was argued, were poisoning the living, and reformers called for the construction of rural cemeteries. The pilgrims who flocked to Ermenonville defined the attitude to death that would culminate in the modern cemetery, the first of which, Père Lachaise, was opened outside Paris in 1804. The new type of cemetery was an Elysian field, a place of rest, a landscape garden where the visitor would commune with the spirit of the departed.

How this institution developed when and where it did is a story that Richard A. Etlín tells superbly. He traces the development of the modern attitude to life and death; he shows how that attitude resulted in competition projects that are of major importance in the history of modern design; and he shows how the rural cemetery emerged from the folds of the aristocratic *ancien régime* and the popular forces of the French Revolution. This is history of a most comprehensive type, ranging from literature, philosophy, politics, and economics to hygiene, architecture, and the history of ideas.

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GUY THUILLIER. *La monnaie en France au début du XIX^e siècle*. Foreword by JEAN TULARD. (Hautes Études Médiévales et Modernes, number 51.) Geneva: Droz, for the École Pratique des Hautes Études, IV Section, Sciences Historiques et Philologiques. 1983. Pp. viii, 450 fr.

Guy Thuillier's intention to rescue the monetary history of early nineteenth-century France from neglect takes on added importance in view of the

well-known partiality of the French for hard coin. In an opening manifesto, he argues for the recognition of monetary history as a separate field of study, rather than as an adjunct to the history of banking, prices, or public finance. Thuillier broadly defines the field of monetary history to include the history of monetary doctrines, institutions, policies, practices, minting techniques, and psychological attitudes toward money. He devotes some attention to all of these aspects of monetary history, and he offers suggestions for further research on many of them.

The essays in this book, most or all of which were first published elsewhere, span the period 1785–1900, but more than three-quarters of the volume deals with the years between 1785 and 1814. Based on original archival research, Thuillier's book demonstrates what fragmentary sources can yield to the imaginative historian. Although this is not a systematic monetary history, as the author recognizes, taken *in toto*, it does illuminate the major events and problems of the era. There are excellent essays on Calonne's monetary reform of 1785, the subsequent reform of the Year XI, and the monetary crisis of 1810. Both Calonne's reform, which changed the ratio of gold to silver (fixed in 1726 at 14.5 to 1) to 15.5 to 1, and the two laws of Germinal in the Year XI, which enticed money out of unproductive hoards to add to circulation, lowered interest rates, and encouraged investment. The Germinal reform fixed the value of the franc (5 grams of silver) and brought unity between money of account and "legal" money. The monetary reforms of 1810, intended to correct certain abuses, led to a series of panics that traversed the countryside until 1813, fueled by rumors that paper money would be issued. With the aid of prefectural reports, Thuillier traces the path of this monetary *grande peur*. Six essays deal with the lively debates over monetary policy during the Revolutionary and Napoleonic periods. Since these essays contribute to an understanding of the earlier essays on monetary policy, it is unfortunate that Thuillier did not integrate them. There are one or two spin-offs only marginally connected with monetary history, such as the interesting essay on four attempts to calculate French national income in the early years of the nineteenth century.

The remaining essays deal with selected nineteenth-century topics: the reminting between 1829 and 1834 of still-circulating coinage from the *ancien régime*; contemporary estimates of the monetary stock around 1840; the partially suppressed Dumas-Colmont report of 1839 condemning the monetary system; an analysis of the speculation in gold and silver in 1857, based on an official inquiry; and the continued persistence of metallic currency at the end of the century. Thuillier agrees with those contemporaries who throughout the century saw

that the French addiction to metallic money, both for exchange and hoarding, inhibited economic growth. What accounts for the persistence of metallic currency? Is the traditional answer that conjures up the specters of John Law and the assignats correct? Thuillier skirts these important questions. In sum, this volume is not quite a monetary history of the period, but it is a good start.

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RACHEL GINNIS FUCHS. *Abandoned Children: Foundlings and Child Welfare in Nineteenth-Century France.* (SUNY Series in Modern European Social History.) Albany: State University of New York Press. 1984. Pp. xvii, 357. Cloth \$47.50, paper \$16.95.

Rachel Ginnis Fuchs's account of the French state's response to the high proportion of abandoned children in nineteenth-century Paris (but only parenthetically in the rest of France) is a thoroughly researched and remarkably evenhanded institutional and social history.

Abandoned Children analyzes and assesses a unique social policy of shaping a docile, productive, family-oriented underclass by means of state-supervised family socialization through foster parentage. The book goes beyond the usual description of the formulation and implementation of legislation to quantitative studies of the impact of legislation on biological and foster mothers and foundlings. Fuchs shows how changing regulations altered patterns of abandonment and foster care and suggests that the introduction of state officials into private homes was a force for behavior modification among the poor. By emphasizing legislators' and bureaucrats' priority of economy over infant lives in the July Monarchy, and by demonstrating how the new state commitment to supervising and saving foundlings reduced infant mortality in the Third Republic, she provides an explanation for the acceptance of Donzelot's "tutelary complex."

Fuchs uses a sophisticated statistical analysis of a random sample of the records of the Hospice des Enfants Assistés and of the reports of the Assistance Publique to produce profiles of foundlings (to the age of twenty-one) and even of birth mothers (90 percent of whom left some kind of identification, despite provisions for anonymity). After proving that about three-quarters of the mothers were single, over twenty years of age, and living alone and working in low-paying feminine occupations after immigrating to the city, she concludes that these women were neither promiscuous nor pitiless, as Shorter argues, but rather lonely, vulnerable, and economically incapable of rearing their children.

Fathers are conspicuously absent, because, Fuchs explains, the civil code prohibited research into paternity and officials were only interested in foster mothers. These and similar policies are interpreted as evidence of social attitudes toward unwed mothers, illegitimate children, and the family.

While the book presents a clear picture of moral condemnation of the mothers (but not once, until the 1880s, of the fathers) and anxiety about the "inherent vice" of the foundlings, the ideals of family life seem indistinct, even contradictory. Quite likely these ideals were vague and strangely bourgeois for peasant families, but the subject deserves more attention. So, too, does the identity of the largely faceless politicians and bureaucrats. Although Fuchs considers the qualifications of wet nurses and field representatives in the countryside (and finds them wanting even by contemporary standards), she does not describe the political affiliations and careers of key legislators or central officials. The reason may be the difficulty in tracing dozens of individuals in personnel files, but the result is an impression of consensus.

In sum, this pioneering study not only adds to our knowledge of the origins of child welfare and of the life of the poor but also suggests new questions about policy making and the family.

MARY LYNN STEWART-MCDOUGALL
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BENJAMIN F. MARTIN. *The Hypocrisy of Justice in the Belle Époque*. Baton Rouge: Louisiana State University Press, 1984. Pp. xii, 251. \$25.00.

This book is an attempt to explore the culture and mentality of the French *belle époque* through a detailed reconstruction of three of the period's most dramatic trials. Arguing that a full evaluation of justice is seldom exhausted by an analysis of verdict and sentence, Benjamin F. Martin proposes to study these celebrated cases from the broadest social and political vantage point and to reckon the quality of justice dispensed in light of the behavior and interests of all of the principal participants, appellants, defendants, witnesses, lawyers, magistrates, and "interested" politicians and journalists. He thus hopes to reveal the same richly interwoven texture of politics, ideology, and personal interest in civilian justice that historians have already demonstrated for the most celebrated military trial of the period, the Dreyfus case.

The three cases Martin examines include two murders, each of which involved some relationship to politicians and political life in this period, and one of the most spectacular confidence schemes of the era, which also provoked reverberations in the political life of the Third Republic. The murders were the mysterious deaths of the society painter

Adolphe Steinheil and his mother-in-law, Emilie Japy, in May 1908, and the shooting of *Le Figaro's* editor, Gaston Calmette, by Henriette Caillaux on March 16, 1914. The chief suspect in the Steinheil case was the wife and daughter of the two victims. Meg Steinheil, a new breed of republican *courtisane* whose charms so agitated the president of the Republic, Félix Faure, that he died in her arms during a liaison at the Elysée palace in 1899. Henriette Caillaux was the wife of the finance minister and former premier Joseph Caillaux. She took her actions, she claimed, to prevent Calmette from publishing love letters between herself and Caillaux that revealed their adulterous relationship prior to their marriage. In both cases, however, there was evidence of political involvement and manipulation in the investigations and trials to "contain" the cases within safe boundaries. The fraud trial of Frédéric and Thérèse Humbert, which unfolded between 1902 and 1903, also reached the highest levels of the republican elite. The ease with which the Humberts built a small fortune of artificial credit out of an imaginary legacy could not have occurred without the compliance or blindness of French police and magistrates.

Martin examines the judicial and police records of these cases to try to determine the motives of officials sworn to uphold the law for interfering with the course of justice. He has found ample evidence for such meddling in all these *affaires* and provides excellent documentary proof of venality of police and magistrates in the Third Republic and of the unashamed political manipulation of civil justice. Martin's narrative accounts of each trial are witty and rapidly paced and cleverly reconstructed to express the melodramatic appeal they provoked in contemporaries.

Despite its modest scholarly apparatus and its resemblance to popular history, this book will be read with profit by students of this period and of the modern criminal justice system. Martin is not attempting a grand synthesis of crime and punishment in the *belle époque*. He makes little effort to identify the major trends in criminality in this period or to analyze the professional outlook or ideological considerations of doctors, magistrates, or lawyers. Nor does he argue that these trials are representative of the way justice was usually dispensed in French courts. But readers will nonetheless learn much of value about French law and legal process, and the forces that influenced the "hypocrisy" of justice while being delightfully entertained.

ROBERT A. NYE
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MICHAEL JABARA CARLEY. *Revolution and Intervention: The French Government and the Russian Civil War*,

1917–1919. Buffalo: McGill-Queen's University Press. 1983. Pp. xiii, 265. \$30.00.

The opening of the French military and foreign ministry archives for the World War I era in 1969 and 1972 led to publications offering new interpretations of French policy toward Germany, East Central Europe, and Russia. These indicate that France's hegemonial aspirations had matched Germany's.

Michael Jabara Carley's monograph shows that French imperialism shaped policy toward Russia from the outset of the March 1917 revolution. French civilian and military leaders considered making deals with Turkey and Germany that aimed to accomplish "a peace on the back of Russia" (p. 16). The French general staff gave up on Russia's continuing war effort, while at the same time it discounted a German breakthrough on the Western Front. Therefore, contrary to conventional views, it was not the fear of German victory over the Entente, but the likelihood of German rather than French economic domination over Russia that inspired intervention.

To prevent German ascendancy in Russia after the Bolshevik takeover, the ministry of war developed a dual policy. It called for contacts with the Bolsheviks in Petrograd and intervention elsewhere. This policy, originating in the foreign ministry, diverged from the desideratum of the general staff, which was in favor of rapprochement with the Bolsheviks in order to bring about military victory against Germany. The Quai d'Orsay, however, had its sight on post-bellum economic domination well beyond the south Russian sphere defined in the Franco-British agreement of December 23, 1917. For this reason it stressed a pragmatic policy that at times favored the Bolsheviks or their political opponents, or at times the independence movement of the non-Russian nationalities. All depended on which group had the potential of resisting German economic influence irrespective of the outcome of the war. Germany's defeat, however, eliminated its economic threat while France sought to exploit Russia (and the Rhineland) for its own recovery.

Ideology, therefore, was not as important in shaping French policy as historians have suggested; nor was the narrow financial interest of French investors in the defunct Russian empire. The economic imperative for intervention was reinforced by crusading anti-Bolshevism only when the Red Army threatened to export the revolution. By the time the French decided on an all-out confrontation in southern Russia in November 1918, they lacked the wherewithal. Thus, an exhausted France, prisoner of its imperialistic tradition, embarked on a doomed policy.

In this important work, this reviewer could quar-

rel only with the author's stress on a consistent polarity between the Quai d'Orsay and the general staff policies. For instance, two notes of Colonel Alphonse Joseph Georges are given as sources for Clemenceau's move against the Red Army (p. 108). In contrast, the foreign ministry is seen as pursuing control of Russian resources by identifying them as "security or collateral for the creditors of the Russian government" (p. 127). Yet the dichotomy becomes blurred with the third note of Colonel Georges of October 30, 1918, not cited by Carley. This note indicates that it was the general staff that first proposed taking collaterals in Russia in order to defuse possible public objection in France to what appeared to be unwarranted French intervention in Russia's internal affairs.

With this said, the monograph is strongly recommended as it convincingly challenges accepted wisdom about the French intervention in Russia.

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PHILIPPE MASSON. *La marine française et la Mer Noire, 1918–1919*. (Série Internationale, number 21; Service Historique de la Marine.) Paris: Publications de la Sorbonne. 1982. Pp. xi, 669. 120 fr.

In a massive and impressive study, based on extensive evidence from the French Naval Archives, Philippe Masson has charted a course through the dangerous reefs of the Black Sea controversy. This is not an easy assignment. From the earliest days, polemics, myth, and distortion have found their way into accounts of the French intervention in southern Russia at the end of the First World War. For Marxist historians the heroic mutinies of French soldiers and sailors foiled the French attempt to overthrow the Bolshevik revolution, a view fostered by the romantic and self-serving accounts of André Marty. For non-Marxist historians, relying largely on sources brought out by the white immigration, the French intervention was a bad-faith effort that left Denikin's Volunteer Army in the lurch. Masson tries to sort out what actually happened, as distinguished from what is supposed to have happened, on the basis of the French archives, ultimately coming down on the non-Marxist side in explaining the circumstances that led to military rebellion and the withdrawal of the French expedition. He gives full justice to the complexity of this confusing series of events.

Masson's study is a double assignment that is divided into three sections. In the first part he analyzes the decision to intervene in the Black Sea and describes the events and subsequent decision to withdraw. This section offers the most complete and balanced account available from the French per-

spective. A confusion of aims, a lack of means, and skepticism over the prospects for success on the part of the military leaders marked French policy from the outset. The military leaders, both Franchet d'Esperey and General Berthelot, despite their personal differences, believed that the Volunteer Army would make the main military effort in southern Russia while French army and naval units provided support. On arrival in the Ukraine, French officers discovered the complexities of political rivalry and infighting among the anti-Bolshevik factions, and they became disgusted with the reactionary views, the weakness, and the social snobbery of the Volunteer Army. Despised for their association with the volunteers, confronted by a surprisingly effective and disciplined pro-Bolshevik resistance, harassed by hostile civilian populations in the Ukrainian ports, and, above all, well aware of the war-weariness of their own troops, the French officers persuaded the government in Paris to cut its losses. In this sad story, into which the French dragged the Greek army and navy as well, Masson rightfully stresses the defeat at Kherson as a turning point leading to the hasty and chaotic evacuation of Odessa in March and, having been stung once, to a second decision to pull out of Sebastopol a month later. Masson severely criticizes the political leadership for sending a military expedition without clearly defining its mission and without providing the resources to carry out any mission that required sustained military operations.

The second and third sections deal with the mutinies at Sebastopol and, later, among units in metropolitan France. Evidence of indiscipline existed from the first, seen in the refusal of two French companies to fight at Kherson, but the best-known episode was the mutiny of French sailors at Sebastopol. Masson notes that there were grounds for complaint relating to poor food, few shore leaves, extra duty, and so on, and that these conditions contributed to low morale, although conditions began to improve by April. He observes that both officers and men hoped for an early return to France. As for the impact of political propaganda, Masson agrees with the French inquest's view that parliamentary debates and political tracts from France had a greater influence on the revolutionary sentiments of certain mutineers than did the local, pro-Soviet propaganda. In any event, the decision to evacuate had been taken prior to the mutinies of April 19–20, 1919.

Some of the more radical leaders called for revolution, but Masson notes that those mutineers who came from the industrial regions of France displayed the discipline associated with industrial strikes. These "industrial-conflict" characteristics were even more apparent as the strike wave spread to Toulon, Brest, and other ports of the French fleet

in the summer of 1919. Here the presence of political agitators and contacts with the trade unions gave a greater political militancy to the mutinies than was present in the Black Sea events.

In a brief section on the Marty affair, Masson reviews the sensational publicity and claims that attended the two courts martial of the future communist deputy. He concludes that the fanfare and heroic mythology generated by the trials surpassed the actual and insignificant role that Marty played in the Black Sea mutinies.

In taking us through these controversial events, Masson does not hesitate to pass judgment on them. No doubt the various legends will continue to thrive among the polemical interpretations, but historians are in his debt for having produced a solidly documented and valuable contribution to our understanding of what happened in the Black Sea at the end of the First World War.

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WILLIAM A. HOISINGTON, JR. *The Casablanca Connection: French Colonial Policy, 1936–1943*. Chapel Hill: University of North Carolina Press. 1984. Pp. xiv, 320. \$27.00.

The few years between the Popular Front government and the Allied landings in North Africa were crucial for Morocco. A burgeoning nationalist movement and the failure of the Vichy experiment effectively nullified efforts to tie the Moroccan protectorate to greater France. *The Casablanca Connection: French Colonial Policy, 1936–1943* lays out the classic colonial power dilemma, a clash between theory and practice, between hopes and realities. Based on the private papers of General Charles Noguès and several other manuscript sources, the book lends a certain drama to the recounting of a failed policy.

Beginning and ending appropriately with the legacy of Marshall Lyautey, the author outlines the theoretical position that limited every resident general in Morocco. Lyautey sought to make Morocco French through a policy of associationism. This entailed building the confidence of natives and the sultan in France's ability to preserve Islamic culture while bringing the benefits of French civilization to the Maghreb. William A. Hoisington, Jr.'s book is an excellent analysis of the circumstances that made this vision an impossible burden to Lyautey's successors.

In the years following World War I, France found it increasingly difficult to deliver a stable prosperity throughout the empire. An indifferent succession of political appointees in the late 1920s and early 1930s had to deal with a collapsing economy that created stress among natives and settlers. It was here that

the dilemma of protectorate versus possession came into play. In an effort to streamline property transactions and criminal justice, the French coerced the Moroccan government into instituting the Berber *dahir* of 1930. The nationalists could usually make the case that settler interests always prevailed. Seizing on the *dahir* as proof that France intended to undermine Islam as well as the sultan's rule, and appealing to hard-pressed artisans, young bourgeois nationalists in the cities inaugurated their movement. There is, of course, a problem inherent in discussing policy from the perspective of French officialdom. Although Hoisington exposes some of the hardships endured by Arabs and Berbers and although he dramatizes their clashes with authorities, he does not explore the structure of the growing nationalist movement itself.

In one of its more pragmatic decisions, the Popular Front asked General Noguès in 1936 to control the situation. Noguès was able to quiet the nationalists, but he did so at the price of direct intervention in the 1937 riots. The nationalists lost a round but made their point. French power ran Morocco. The Lyautey formula was proving useless. Still, in Noguès's mind the nationalists could be outflanked, the indigenous population made prosperous, and Morocco made a monument to French civilization through an elaborate system of state aid to native farmers and artisans.

These economic difficulties might have been overcome as long as Noguès commanded power and resources. France's defeat in 1940 and the German occupation of Vichy in 1942 deprived him, in large measure, of both. Noguès was no rebel. Along with many Vichy officers in the colonies, he was anti-German. He was not alone in his belief, however, that he could reconcile commitments to the Maghreb with loyalty to Vichy.

The author has managed a fresh, sensitive view of Vichy machinations in North Africa. And he has outlined carefully Noguès's decision to oppose the American landings. Georges Mandel, General Weygand, and Admiral Darlan—Noguès survived them all. Only General Giraud was able to slink into the Gaullist camp. Noguès was removed in June 1943 and later stigmatized with "national degradation." A colonial officer to the bitter end, he interpreted events in purely Moroccan terms. He remarked finally that the protectorate was finished only if it could not count on the sultan's cooperation. Without becoming an advocate, yet with some sympathy, Hoisington allows that historical misperceptions and personal style could lead a loyal soldier like Charles Noguès to pursue a lost cause in the belief that he served his country and himself.

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WILL MORRISEY. *Reflections on de Gaulle: Political Founding in Modernity*. Lanham, Md.: University Press of America. 1983. Pp. x, 211. Cloth \$22.75, paper \$11.50.

Will Morrissey's *Reflections on de Gaulle* is an unusual study that will be disappointing to historians seeking new insights into the intentions, character, or behavior of Charles de Gaulle. The book is divided into five parts within which the author sets out a textual interpretation of de Gaulle's major writings (in the order of their publication), which are then juxtaposed with the thoughts of numerous political philosophers, including Plato, Aristotle, Machiavelli, Locke, Hobbes, Montesquieu, and Hegel. The reader is referred to writers as diverse as Clausewitz, Nietzsche, Tocqueville, Lenin, and Jefferson, with an occasional quotation from contemporary political scientists or historians and even André Malraux's funeral oration on the occasion of the transfer of Jean Moulin's ashes to the Pantheon. The writer invites his readers to participate (one of the Gaullist notions he emphasizes) in a dialogue with de Gaulle (the host of writers he quotes) and the author himself, suggesting that "perhaps the most important contributor of all is the reader, the dialogues' silent partner" (p. ix). This statement from the author's preface suggests what seems to me to be a major problem with the book.

Beyond attempts to classify de Gaulle's thought as "modern" or "classical" (ancient or traditional), it is difficult to discern an interpretation except for the author's obvious conclusion that there were elements of both modernity and tradition in de Gaulle's ideas. The author indicates that his reflections on this plethora of writers derives from "themes intimated by de Gaulle" (p. ix), but some seem much closer to de Gaulle's ideas than others. On many occasions the reader is not informed whether de Gaulle actually read or referred directly to the thinkers presented for reflection, and overall, the impression created reminds one of a game of charades, with participants stating: "Sounds like Montesquieu," "Sounds like Hobbes," and so on. For the most part the reader is offered analogy as a substitute for analysis.

For a reader unfamiliar with the writings of de Gaulle this book might serve as a useful summary of the major writings. Yet, these are reflections on de Gaulle more in the sense of a mirror image than in terms of critical interpretation. The author presents de Gaulle, French politics, and the French in Gaullist terms, accepted more-or-less at face value. Claiming, without adequate demonstration, that de Gaulle's plans and ambitions for France were fixed from the mid-thirties, Morrissey rejects the interpretation of Stanley Hoffmann (whose work he cites approvingly on other points) who describes a gradu-

al broadening over time of the Gaullist vision. The author denies "the utility of any simple historicism in understanding de Gaulle" (p. 113). Perhaps because the book is based almost entirely on the published texts of the writers discussed, with selected references to secondary literature, the author fails to acknowledge, for example, the influence of resisters in France on de Gaulle's outspoken republicanism during the Second World War and accepts without reservation de Gaulle's own dating of his recognition that the French empire was finished. To this reader it seems odd that the author, who insists on de Gaulle's attention to circumstances and the necessity of flexibility to deal with them, seems to think de Gaulle's ideas were so little influenced by changing circumstances.

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ESTEBAN HERNÁNDEZ ESTEVE. *Creación del Consejo de Hacienda de Castilla, 1523-1525*. (Estudios de Historia Económica, number 9.) Madrid: Banco de España. 1983. Pp. 216. 500 ptas.

In the context of the lively interest in Spanish Habsburg finances touched off in 1949-67 by Ramón Carande's great trilogy on royal revenues, state fiscal machinery, and recurrent deficit crises under Charles V, this illuminating monograph focuses on a crucial early phase—the inauguration after the emperor's return to Spain in 1522 of the henceforth permanent *Consejo de Hacienda*, or Council of Finances. It has long been known that this small body of trusted high officials dates from 1523 and that it was charged with imposing badly needed unifying control over the revenue collecting and disbursing operations of the monarchy's two old, mutually independent, and equally inefficient exchequer departments, the *Contadurías Mayores* of *Hacienda* and of *Cuentas*. Esteban Hernández Esteve, already known for a book and other studies dealing with sixteenth-century Spanish accounting practices, shows here from newly discovered documentation at Simancas that this "constituent" process involved much more than a simple royal act of creation: the agency that we know emerged in definitive form only after some two years of complicated institutional experimentation and personnel shifts.

Following the emperor's original (lost) directive of early 1523, three major turning points in the formative process can be fixed on the basis of the successive royal *Ordenanzas* of February 1523, March 6, 1524, and January 1525. In his second and third chapters, the core of the study, Hernández Esteve meticulously analyzes these texts, along with the *Instrucciones* simultaneously addressed to the comptroller (*tesorero general*). He shows how these texts

chart both the evolution of the council's structure and personnel, and the steady passage of control from the hands of the panimperial-minded Grand Chancellor Gattinara to the more pragmatic State Secretary Francisco de los Cobos, head of the Castilian-nationalizing faction. A valuable preliminary chapter surveys in detail the sources and conclusions of the six principal students of the subject, and a terminal chapter offers fresh confirmation of the long-espoused view that the Castilian *Consejo de Hacienda* was modeled on the Flemish *Conseil des Finances* (although, as Esteve insists, only after significant adaptations to Castilian exchequer tradition).

Something more might have been done to clarify the new agency's relations with the *tesorero* and the two strongly surviving *Contadurías Mayores*; and close collation with the simultaneous establishment in 1524 of the partially financial Council of the Indies could have provided helpful comparative light. In any case, this is a welcome contribution to understanding the genesis of a major cog in the creaking fiscal mechanism of Habsburg Spain. It also supplies additional reasons for questioning (as against Karl Brandt and, more recently, J. M. Headley) Gattinara's potency in the shaping of the Caroline bureaucratic state.

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HARRY VAN VELTHOVEN. *De Vlaamse Kwestie, 1830-1914: Macht en Ommacht van de Vlaamsgezindheden* [The Flemish Question, 1830-1914: Power and Impotence of the Flemish Group]. (Anciens Pays et Assemblées D'États, number 82.) Kortrijk-Heule, Belgium: UGA. 1982. Pp. 369.

The amount of literature on the Flemish question is rather voluminous and probably does not need many additional monographs. This work, however, attempts to throw light on a much neglected phase of the Flemish question, the impact of the Flemings on the major Belgian political parties in the nineteenth century.

The Flemish movement was initiated in the early part of the nineteenth century by literary men who reacted against attempts by the ruling French-speaking elite to frenchify the nation. Initially, the Flemish movement was petty bourgeois and had very little support from the Catholic Flemish population. Nor did it have contact with the Netherlands to the north. It was not until the 1860s that the Flemish movement began to take part in politics and demanded not only protection of the Flemish tongue but also equality. As a result, parliament passed a number of language laws in the 1870s and 1880s that allowed Flemish, or Dutch, a larger part

in jurisdiction, administration, and secondary education. These laws, however, were mostly ignored.

During the latter part of the nineteenth century a Flemish nationalism emerged that demanded that Flanders should become exclusively Flemish. In 1898, after an intense three-year struggle, parliament passed a law making the Dutch version of the laws legally valid. This episode for the first time aroused a broad segment of the Flemish population. Yet this success was only the beginning of an intense and frustrating struggle for even greater linguistic and cultural equality that would end in failure in the years before 1914.

This monograph focuses principally on the interaction between the Flemish movement and the major political parties from 1890 to 1914. It is based on a vast amount of archival and printed sources and was originally submitted as a doctoral dissertation. The author describes in some detail the evolution of the Belgian political, constitutional, and electoral system and explains why the major political parties—the Liberals, Catholics, and Socialists—were not much interested in the Flemish movement. The Liberals were dominated by the French-speaking middle class while the Catholics, who were to represent the interests of the Catholic Flemish population, identified themselves with the Belgian state. The Socialists failed to respond to the Flamings' demands because they felt that the Flemish question would distract too much from the important social and economic issues.

The principal chapters describe in much detail the reactions of these three parties to the language laws introduced between 1901 and 1914 and the proposal to "dutchify" the University of Ghent. All efforts to secure passage of these measures and to change the university failed, and we see how and why the major parties failed to respond. All of this is a fascinating story, and we learn much about Belgian political history. Yet, this work suffers from a lack of lucidity. Even for those who have some familiarity with all of the complexities of the Flemish question, it is not easy to follow the author's accounts of the many legislative and political struggles, and this reviewer had to reread many a passage in order to understand the narrative. In addition to the lack of clarity, this reviewer was irked by the large number of anglicisms. Who ever said that the Netherlandish tongue has been preserved in a more pristine state in Belgium? To me the dutchified anglicisms *okkasioneel*, *kruciaal*, *initiele*, *item*, *kontekst*, *kulmineerde*, and so on sounded a bit ludicrous, if not absurd.

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DANIEL H. THOMAS. *The Guarantee of Belgian Independence and Neutrality in European Diplomacy, 1830's—*

1930's. Kingston, R.I.: D. H. Thomas. 1983. Pp. xii, 789. \$35.00.

Fifty years ago, the lead article of this journal (*AHR*, 39 [1933]) by William E. Lingelbach was an analysis of Belgian neutrality. A half century later, Daniel H. Thomas, a Lingelbach student, has himself written and published a long-awaited full treatment on that subject. Although monographs by Horst Lademacher and Jonathan Helmreich have recently ploughed this turf with great success, this book of nearly six hundred pages constitutes the most competent, thorough, and balanced work on the small state's diplomacy, not merely in interaction with the five great powers of the nineteenth century but also through the crucial aspects of international law and the interplay of domestic affairs.

From its modern birth in 1830 until its abrogation of neutrality in 1920, Belgium is depicted by Thomas as living with the inseparability of its national independence from its neutral status. The role of that guarantee in the European state system is drawn in three chronological eras by the author. Unhappily, he minimizes the critical question of interwar alliances and the 1936 policy of "independence." The discussion of early nationhood and the beginning of neutralization with the agreement of the Big Five to guarantee an independent, neutralized Belgium in 1839 contains few new points, but Thomas does outline Palmerston's dominance over Talleyrand and Metternich. There could be a greater recognition of Palmerston's use of the Lowlands to move beyond Vienna in creating an alternative diplomatic structure and, equally, a clearer perception of the Metternichean wish to see Belgium become an issue between Paris and London. Yet, Thomas outlines with steadiness and clarity the leading British stance—that neutrality be a permanent reality in contrast to the "expedient and makeshift" character that it had prior to mid-century for the other powers. Seldom have the struggles of Channel and Eastern powers appeared so vividly.

The analysis of the middle period, from Napoleon III to the Great War, is a model of diplomatic history. To Thomas, the 1850s and 1860s are principally the time of Belgium's foremost industrial prowess and its greatest international vulnerability. The hows and whys of the guarantees of survival in the face of French ambitions and Prussian growth are cleverly illustrated by the escape from such threats as the Luxemburg crisis of 1867. The insights of the post-1871 period are equally important and noteworthy, ranging from the comprehensive chapter on the international legal background (the best found anywhere, at least in English) to the well-argued dissection of the little-known 1887 war crisis. The strengths of the military policy sections make

the book more worthwhile, for the depth of Thomas's probe into the evolving inadequacy of Belgium's defense is found nowhere else. His integration of the Madame Adam ("Count Vassili") affair of 1888, press reactions, and Belgian Liberal-Catholic party squabbles is successful history.

The work terminates with a major accomplishment and a shortcoming. Thomas's discussion of the origins of the First World War makes for detailed, fascinating reading. His assessment that the guarantee served Belgium well until Versailles leads him to argue that neutrality would have served the country better than alliances. These conclusions are weakened by the lack of thoroughness on this era. One needs the details of Sally Marks, David Kieft, or the most recent work by Fernand Vanlangenhove to complete the picture.

There are three additional points of significance. First, Thomas revises the roles of the monarchs, Leopold I and II, and major advisers like Stockmar in foreign policy formation. Second, his research covers Austrian, Belgian, Dutch, British, French, and German published and unpublished materials with admirable intelligence. He makes private papers like the Van de Weyer and Lambermont collections a valuable part of European relations. And lastly, even though the research began forty years ago and Thomas's first article appeared more than thirty years ago, the wait for many of us was well worth it. A lifetime of effort has resulted in a real contribution to our understanding.

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FOLKERT POSTMA. *Viglius van Aytta als humanist en diplomaat, 1507-1549* [Viglius van Aytta as Humanist and Diplomat, 1507-49]. Zutphen, The Netherlands: De Walburg, 1983. Pp. 239. f 36.

Viglius was educated in the Erasmian *Collegium Trilingue* in Louvain, and he admired Erasmus above all his contemporaries, but, our author tells us, he "never became an Erasmian *pur sang*" (p. 228). Not content with scholarship, he sought an opportunity to enter public service. In 1542 he became a member of the Secret Council of Charles V's regent in the Netherlands. Mastering the law of the empire and diplomatic precedent, he made himself indispensable to the Habsburg rulers of the Netherlands and occupied a position at the center of power in their Councils, under Charles V and under Philip II, until his death in 1577. This is a story that no one, until now, has undertaken to work out from the documentary records. (See F. H. Waterbolk's historiographical survey of scholarship on Viglius, *Rond Viglius van Aytta*, 1980 [pp. 7-10].)

Earlier historians have often seen *letvados* like Viglius as mere creatures of the king. H. A. Enno van Gelder offered Viglius as a prime illustration of his contention that the cause of Philip II was upheld in the Netherlands not just by Spaniards, but by Netherlands who believed in monarchical principles. Pieter Geyl, on the other hand, describing the three members of the Council on whom Philip particularly relied, called attention to their non-Dutch character: to the fact that Granvelle was a Comtois, Berlaymont a Walloon, and Viglius a Frisian. Such men, he asserted, "knew no law but the king's will and waited for instructions direct from Madrid regarding the policy their country should follow" (*The Revolt of the Netherlands* [1962], p. 70).

The present volume, however, deals with an earlier period in the life of Viglius, corresponding roughly to the reign of Charles V, when few Netherlands (or Frisians) were thinking in terms of fundamental conflict between crown and country. After an opening chapter on Viglius's humanist education and juridical scholarship, and a chapter on Viglius's career in Germany (1534-42) as assessor on the Reichskammergericht and as professor and eventually rector at the University of Ingolstadt, the remaining two chapters are devoted to Viglius's career as a public servant in the Netherlands to 1549. (A sequel is promised that will carry the story to 1577.)

Folkert Postma shows that from 1542 to 1544, when Cleves was the hinge of a great anti-Habsburg alliance, Viglius was repeatedly the one chosen to present the Habsburg point of view to the Reichstag. In the next period, 1544-49, in the aftermath of Charles's triumph over the Schmalkaldic League, the relationship of the Netherlands to the empire became a major issue. It was settled by the Treaty of Augsburg (June 26, 1548), which recognized the internal autonomy of the Netherlands. This was not only a triumph for the interests of the Habsburg dynasty, but also for those of the Netherlands, skillfully defended by Viglius against what were on occasion contrary attitudes on the part of the regent or the emperor.

Viglius's career would seem to call for reflections on the relation between his humanist ideals and his public service. Comparisons with Agricola and Erasmus would be of interest. For Viglius, whose Frisian homeland had only recently been added to the other Netherlands possessions of Charles V, what was the *res publica* that he should serve—Frisia, the Netherlands, or the empire? The author does not discuss such problems, and he was probably right to refrain from doing so in this volume. In the sequel, after Postma has dealt with the period in which Viglius continued to serve in the council under Philip II, and even under Alva, until he was arrest-

ed—along with the other members—by the “patriots” in 1576, it will be time to offer a reassessment of Viglius’s patriotism in the perspective of his whole career.

GORDON GRIFFITHS
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J. M. M. DE MEERE. *Economische ontwikkeling en levensstandaard in Nederland gedurende de eerste helft van de negentiende eeuw* [Economic Development and Standard of Living in the Netherlands during the First Half of the Nineteenth Century]. (Cahiers Sociale Geschiedenis, number 1.) The Hague: Martinus Nijhoff. 1982. Pp. 144. f. 27.50.

The first quarter of this book by J. M. M. De Meere addresses the question of economic growth or stagnation in Holland during the first half of the nineteenth century. It starts from the existing literature, which in the traditional perspective of an English standard and model of the Industrial Revolution has always described the Dutch case as one of stagnation. As long as we continue to associate industrialization with urbanization, the reader actually finds justification for the accepted view in the latter part of this book. Numerical data show that the level of urbanization in the Dutch provinces on the whole stagnated in the period 1795–1840. The least urbanized provinces experienced some urbanization, but the most urbanized provinces (North Holland, for instance) experienced relative deurbanization. So revising of our perspective on Dutch economic modernization is the outcome of an analytical separation of the processes of economic growth, industrialization, and urbanization. The need for such a separation is something that has been noted before and keeps being confirmed by new research as the industrialization or urbanization of one European country after the other comes under new scrutiny.

De Meere observes that the available data generally indicate that the most important sectors of the economy grew faster than the Dutch population after 1825–30, indicating positive per capita economic growth rather than stagnation. This conclusion seems warranted, but it should be pointed out that De Meere’s arguments and findings are heavily weighted by data, calculations, and observations on the period 1817–70, a slice of time that is rather off-center from the point of view of the “first half” of the nineteenth century.

Three-quarters of the book is devoted to the study of issues of income, employment, and wealth, of different social groups in the population in the period, between the great crises of 1817 and 1846–47. Attention is concentrated on a study of the labor market and on the phenomenon of the seasonal

immigration of foreign workers, on estimating structural unemployment, on evaluating the trend of income distribution (it became less uneven), on assessing the trend in the number of paupers, on measuring the wage level of different provinces and occupational groups, and on gauging the food consumption patterns of low-income rural and urban populations in relation to mortality.

This is an attempt to use a variety of disparate sources to draw a composite picture of economic and social change, or at least to show how such a composite picture could be painted in the future. The author calls the book a “cahier,” a sketch book, and repeatedly warns us that it should be viewed as a report of work in progress, giving out the first results of ongoing research, rather than a finished and definitive product. The book is also a call for further research by others, and it demonstrates what use can be made of certain archival sources hitherto little used. Its unfinished nature being thus recognized, this book is welcome as a stimulating, refreshing, and useful work.

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BJÖRN ASKER. *Officerarna och det svenska samhället, 1650–1700* [Officeholders and Swedish Society, 1650–1700]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensis, number 133.) Uppsala: Uppsala Universitat; distributed by Almqvist and Wiksell International, Stockholm. 1983. Pp. vii, 214. 105 KR.

Numerous recent Swedish historical works have been written on the development of a centralized, absolutist state during the seventeenth century and the consequent decline of the aristocratic elite that had previously been so influential. Björn Asker contributes to this scholarship by examining the recruitment and promotion of Swedish military officers from 1650 to 1700.

As a general premise, the author maintains that the development and expansion of the Swedish empire required more competent officials than the old aristocratic order could provide. The aristocracy was therefore forced to admit into the bureaucracy many who lacked the social or economic distinction they themselves enjoyed. But since it was also to their advantage to see the state run well, they were compelled to share the rewards of power with this new group. The power and influence of the old aristocrats therefore declined, and eventually they were unable to prevent the newly ennobled bureaucrats from leading the successful attack on their most cherished rights and privileges.

Using military records as well as family and

individual biographies, Asker then seeks confirmation of this thesis in careers of Swedish military officers. One sign of the aristocracy's decline can be seen in the tendency for them to fall back on military appointments as their influence on the civilian administration faded. Nevertheless, statistical data indicate that even then they increasingly had to accept positions within lower ranks of the officer corps, because their hold on the top military positions also slackened throughout the century and, given the increase in the size of the army, their share of officer appointments declined.

The real winners in the contest for appointment as military officers by the end of the century were the "new nobility," for they had increased their share of positions at the expense not only of the aristocrats but also of the peasantry and commoners, who now possessed fewer chances than before for selection to even the lowest officer grades. It was also apparently the sons of the civilian bureaucracy who most frequently became a part of the officer corps and received promotions instead of the sons of their military counterparts.

The statistical data presented by Asker fails to substantiate his conclusion that no new elite emerged after the decline of the aristocracy. The material gathered by the author seems to suggest, however, that the bureaucratic "new nobility" did supplant the aristocracy as a new ruling class. Asker might well have chosen to explain his contention more thoroughly or stated it less absolutely. The author presents his study in a well-ordered narrative accompanied by numerous tables, most of which he wisely relegates to an appendix while integrating the rest effectively into the text. He only falters once in this regard while discussing the comparative length of military careers. His reference then to tables both in the text and appendix leads only to confusion.

These are minor problems in a book that is otherwise an enlightening and useful addition to the expanding study of seventeenth-century Sweden.

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PAUL-JOACHIM HEINIG. *Reichsstädte, freie Städte und Königtum, 1389–1450: Ein Beitrag zur deutschen Verfassungsgeschichte*. (Beiträge zur Sozial- und Verfassungsgeschichte des Alten Reichs, number 3.) Wiesbaden: Franz Steiner. 1983. Pp. viii, 439. DM 98.

Paul-Joachim Heinig has composed an exhaustive review of the relations between the German king and the towns constitutionally dependent on him between the end of the great urban war of 1388 and the second urban war against Albrecht Achilles in 1450. It is part of a growing effort among scholars of all nations to study the Reich, and all premodern

political communities, without the constraints of either nationalism or corporatism. It is hard to say what Heinig wants to put in the place of these constructs, but he has certainly gathered the materials for an analysis.

He commences by selecting four imperial towns (*Reichsstädte*, towns considered part of the royal demesne), Frankfurt am Main, Augsburg, Nuremberg, and Ulm; along with four "free towns" (*freie Städte*, former episcopal towns that had obtained autonomy guaranteed by the king), Mainz, Cologne, Regensburg, and Strasbourg. He examines royal charters and letters that survive for these towns from the chanceries of Wenzel, Rupert, Sigismund, Albert II, and the start of the rule of Frederick III. He reviews these documents under the headings: the functions of the towns for the king, the citizens for the king, the king for the towns, and the king for the citizens. He argues that the intensity and quality of relationships between the king and the towns varied with the proximity and accessibility of the king in a particular political situation. At the end of the story, the reign of Frederick III saw the virtual abandonment of the traditional cultivation of towns and townsmen, a link only to be revived and fostered by different means by Maximilian I.

Much demonstrated here should be familiar to the students of other late medieval polities, even those much less complex than the old Reich. The assignment of revenues from towns, which amounted to over eleven thousand guilders per year in the period 1400 to 1410, is shown to be standard operating procedure in premodern finance, not a perversion of the principles of the Reich. It is most important to notice that the relationships with the king were direct, not mediated or mitigated by other Reich institutions. The final defeat of towns in the middle of the fifteenth century would turn Germany into a republic of princes, with the towns as defensively huddled exceptions to the rule. In the aftermath of that defeat, the Reichstag would emerge as it was to exist throughout the old Reich, a corporate body representing the Reich as a force distinct from the king and his servants.

Heinig has produced the basic data for a reexamination of the late medieval Reich cleansed of the polemic wars of the last century. It is quite well written in its individual parts, though its insights come in brief flashes at irregular intervals. It makes a good try at portraying a complex late medieval society that should be familiar to those who have worked on French or English sociopolitical life. Although short on positive formulations of its own, this book will surely assist in the process of formulating a general reappraisal of late medieval state life.

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PETER NEWMAN BROOKS, editor. *Seven-Headed Luther: Essays in Commemoration of a Quincentenary, 1483–1983*. New York: Clarendon Press of Oxford University Press. 1983. Pp. xii, 325. \$47.50.

This volume takes its form from Johannes Cochlaeus's vitriolic anti-Luther treatise of 1529, *Septiceps Lutherus*, and from the anonymous cartoon that prefaced it. Author and artist depicted the reformer as a seven-headed apocalyptic beast, a fitting Catholic rejoinder to one who had called the pope the Antichrist and the whore of Babylon. These essays treat the seven heads and other topics too, for, as Peter Newman Brooks notes in his preface, "A mere seven [there are actually eight: two for the middle head] essays explaining the 'heads' render scant justice to the achievement of Doctor Martin Luther" (p. viii).

The uninitiated ought to begin after the middle, with Gotthelf Wiedermann's helpful essay on Cochlaeus himself, "Cochlaeus as a Polemicist." Chaplain to Duke George of Saxony, Cochlaeus had read very few of Luther's works, and fewer all the way through. *Septiceps Lutherus* was designed to spare Catholic priests the need to study Luther's heresies but to enable them nonetheless to refute his teachings. Cochlaeus showed Luther's various "heads," that is, his doctrines, to be inconsistent and conflicting.

Brooks's own essay on the sixth head, "Luther as Visitor," could well be read second, for it best summarizes what Cochlaeus actually said in his polemic. Cochlaeus was originally an Erasmian. Brooks says, "In his dealings with Luther, he may be seen as a continental counterpart of the dark side of Thomas More who so savagely indicted William Tyndale" (p. 159). Cochlaeus derided Luther for his apparently shifting views on *auctoritas* within the church. Luther had at first left order and conversion up to the power of the Word but in 1528, the year before *Septiceps*, had cooperated with the electoral visitation.

The remaining articles reflect the contributors' varying orientations. B. A. Gerrish's "Subjectivity and Doctrine" is intellectual history, giving us in brief Ernst W. Zeeden's, Joseph Lortz's, Paul Hacker's, Friedrich Schleiermacher's, Walther von Loewenich's, and others' opinions of Luther. According to Gerrish, "The title 'doctor' or 'teacher' probably comes nearer than any other to the heart of Luther's self-understanding" (p. 4).

Norman E. Nagel's "'Heresy, Doctor Luther, Heresy!' The Person and Work of Christ" is to this social historian opaque and occasionally unidiomatic. It deals with the relationship between Christ's human and divine natures. Luther upheld the union of two distinct natures and harked ever back to the baby in the manger who "sucks, is washed, and dies" (p. 48).

Martin Schwarz Lausten's "Luther and the Princes" addresses the recurrent question of whether Luther "sold out" to the princes. Lausten answers in the negative but provides no new arguments.

On the subject of Cochlaeus's fourth and central head, Luther as ecclesiast, Robert H. Fischer has demonstrated his sensitivity to historical methodology in illuminating Luther's thought. In "Dr. Martin Luther, Churchman: A Theologian's Viewpoint," he finds Luther's utterances from Coburg in 1530 to reveal a maturation and consolidation of his teachings on church life. Stimulated by the Augsburg Confession, Luther wrote the clearest formulation of his doctrine of the two kingdoms. Fischer is lucid and convincing. Also writing on Luther as ecclesiast is Lewis W. Spitz. In "Luther Ecclesiast: An Historian's Angle," Spitz asserts that "Luther's was a theology with unity, coherence, and special emphasis" (p. 108). This article is at base an encomium and does not appear to employ methodology that is specifically "an historian's." Spitz denies that Luther consented to the use of coercion in organizing an evangelical church.

Mark U. Edwards, Jr., in "Luther's Own Fanatics," gives us a taste of the techniques and insights of his two recent books, *Luther and the False Brethren* (1975) and *Luther's Last Battles* (1983). He correctly insists on seeing Luther's statements against the background of personalities and circumstances. Edwards analyzes the reformer's relations with sacramentarian leaders and finds that he lumped these opponents together, making few distinctions among their motives or arguments.

Gerald Strauss, in "Luther as Barrabas," searches for "Luther's authentic voice on the vital matter of law and obedience" (p. 168). Was Luther Barrabas, the anarchical activist, as Cochlaeus maintained? Strauss concludes that Luther had both negative and positive attitudes toward the law, which he regarded as necessary in any case to keep the *Pöbel* within bounds. But Luther "did not wish to see Barrabas silenced; he only wished to take away his club" (p. 175).

The concluding chapters in this volume treat a variety of themes. Heinz Bluhm, in "Luther's German Bible," affirms that Luther's translation is one of the greatest literary works of the Western world, one that has influenced translations into other vernaculars. University teachers will make use of Ulrich Michael Kremer's "Martin Luther in the Perspective of Historiography," an abbreviated introduction to historians' treatment of Luther and the Reformation from the sixteenth century to the present. In "The Image of Myth and Reality," Leif Grane explains, as though to Cochlaeus, that Luther only appeared to contradict himself as he responded to changing circumstances. Gordon Rupp, in "Luther against 'The Turk, the Pope, and the Devil,'" intro-

duces us to the enemies of Luther's last years, the Turk, the pope, "Hans Wurst," the Fanatics, and the Epicureans. Finally we come to Christian Pearson's "Line upon line: here a little, there a little": Some Letters of Martin Luther." It is rewarding to see a Catholic scholar discover Luther's correspondence and derive as much pleasure from it as others of us did on first reading Luther's letters to Spalatin and to his family.

The editor of *Seven-Headed Luther* achieves a greater degree of thematic unity and over all a higher scholarly standard than is usual in the difficult genre of *Festschriften*. This volume will hold its own alongside German and Scandinavian quinquennial publications.

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JAMES ALLEN VANN, *The Making of a State: Württemberg, 1593–1793*. Ithaca: Cornell University Press. 1984. Pp. 321. \$29.50.

The study of early modern Germany, the Reformation excepted, is still in its infancy in the United States. It appears, however, that with the publication of *The Making of a State* it has reached a remarkable degree of maturity. Perhaps the idea that the Holy Roman Empire disappeared as a viable and effective polity after the convulsions of the Reformation has taken roots too deep in the popular historical imagination for interest in its later centuries to catch on. Or, possibly—and the reviewer suspects that this is the real reason for the neglect—the history of such an irregular polity, to use Samuel Pufendorf's description, may be too complex for minds attuned to the comparative homogeneity of the modern nation-state. If the latter be the case, the work before us is surely the best example of what a skillful and agile intellect can produce out of the most confusing data.

James Allen Vann's work claims to be a reinterpretation of state building in early modern Europe. This assertion may be a bit too broad. After all, the author by his own confession only deals with a power of the secondary tier within the Holy Roman Empire. But even if Württemberg cannot be made a paradigm for the whole of Europe, it is the methodology that Vann employs that deserves wider emulation. Histories of Württemberg are common enough. Their interpretations vary slightly and, in this respect, Vann's book would be no exception and therefore not exceptional. What the author does is not to give us a revisionist history per se, but a whole new vision of Württemberg's story. How did he go about his task? Like his predecessors, he worked through the mountains of records at the archives at Stuttgart, but, unlike them, he breathed life into the dry facts of council meetings and official correspon-

dence. He recovered the human dimensions behind the personalities and, thereby, in a series of deft maneuvers, gives us a social history within the matrix of institutional and administrative history. The result is most enthralling. Admittedly, others had already attempted this combination before him—I can think of Thomas Brady in the case of Reformation Strasbourg—but I know of no one who sustained this intricate exercise for such an extended period, a skill that can perhaps best be compared to that of a juggler who keeps several balls in the air simultaneously without ever dropping one.

Vann's account covers the reigns of several dukes of Württemberg, beginning with Friedrich I (1593–1608) and ending with Carl Eugen (1737–93). In some ways, however, it might be more to the point to say that he treats the period between the two great constitutional documents of Württemberg history, the *Tübinger Vertrag* of 1514 and the *Erbvergleich* of 1770. Within that frame he isolates three important elements, the balance of which determined the nature of Württemberg government. These were the prince, the bureaucracy (or, more narrowly, the privy council), and the territorial estates. He then proceeds to trace the interplay of these three and their changing roles in the state. While engaged in this intricate exercise, the author formulates not only the fluctuations of their official relationships but also their social and cultural ones, putting particular emphasis on the motivations of the rulers and the provenance of his councilors. As if that were not enough, he concurrently explains the divisions within the territorial estates themselves, first between the rural and urban segments, then among the representatives of the urban patriciate (the *Ehrbarkeit*) when the spokesmen for the more dominant towns, together with the Lutheran prelate, began to monopolize the executive committee of the diet, which in turn had superseded the diet as organ of the estates. The influence of the urban estates rose in importance as the privy councilors, who were losing their standing with the dukes at a time when their growing absolutist stance relied on narrow cabinet government, became increasingly interrelated with the families of these executive-committee members. One other element was finally added to the picture, namely, the power of the emperor or, perhaps more properly, that of the Holy Roman Empire. Since the Peace of Westphalia, the latter has frequently been dismissed either as too enfeebled or as being automatically on the side of the territorial princes. On the contrary, Vann demonstrates that in Württemberg the Empire formed a significant barrier to the slide toward princely absolutism by espousing the cause of the local estates. The same could be said for several other of the secondary units within the Empire.

The book has a number of minor blemishes, among them several printing errors. Niccolò Jomelli (p. 269) never made his reputation as a singer. This reviewer's major objection, however, is to the loose use of the term "baroque monarchy," since no acceptable definition for "baroque," unless it be for art, has yet been arrived at. He would also like to take this opportunity to plead for some standardization of English terminology for German institutions. To take one example, it seems every author uses a different word for *Reichskammergericht*.

But all these matters do not detract from the high quality of the work. One can only hope that the book will find a wide readership among historians, not because of the intrinsic importance of Württemberg, but because of the almost novel approach of the author and the excellence with which he carried his thesis to a conclusion.

HANNS GROSS
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STEFAN HARTMANN. *Die Beziehungen Preussens zu Dänemark von 1688 bis 1789*. (Neue Forschungen zur Brandenburg-Preussischen Geschichte, number 3.) Cologne: Böhlau. 1983. Pp. xxii, 402. DM 110.

Stefan Hartmann traces the development of diplomatic relations between Prussia and Denmark during the reigns of Frederick I and Frederick William I (1688–1740) in the first part, and from the ascension of Frederick II to the outbreak of the French Revolution (1740–1789) in the second part. In spite of the change of rulers, what strikes the reader is the continuity of concerns and policy. The inextricable linkage of foreign and domestic policies made any rapprochement between the two powers difficult. The strength of the Danish fleet, its domination of the sound, and its exaction of tolls stifled the development of Prussian trade, especially in cereals. The divergence of interests meant that relations were bedeviled by mutual suspicion and distrust throughout the period. The diametrical concerns of the two states could not be offset by the tie of the *corpus evangelicorum*.

The continuity of concerns forces us to reappraise the traditional interpretation of the vagaries of the first Prussian king and the bungling of the second. Both rulers had a sound and realistic understanding of Prussian limitations and aims. Frederick I's prudence and neutrality in the North should not, Hartmann argues, be interpreted as weakness. The vulnerability of his lands, the ever-present threat of a two-front war, and the priority of his politics in the West made Prussian neutrality a natural and defensive maneuver in the uncertain game of the Northern War. Relations with Denmark, which attempted to pull Prussia into the anti-Swedish coalition, deter-

riorated when Frederick continued to maintain a strict neutrality. Only when freed from the dangers of a two-front war by the conclusion of the War of the Spanish Succession in the West could his successor, Frederick William I, conclude an alliance with Russia and enter the northern melee.

Nonetheless, relations continued to be difficult. Denmark increasingly feared the military might of Prussia under Frederick II. On his part, Frederick II saw Denmark as an integral element in the maintenance of the status quo in the North, a balance endangered by the preponderance of Russia.

Hartmann extends the study beyond relations with Denmark by integrating the Northern question into larger European problems such as the War of the Spanish Succession, the quarrels with Hanover, the Jülich-Berg question, and the seizure of Silesia. This tightly argued book, geared for the specialist, represents the best of traditional diplomatic history. Although the tale is told mostly from the Prussian viewpoint, Hartmann has made a thorough use of secondary sources and of the archival material in the Zentrales Staatsarchiv in Merseburg and the Reicharchiv in Copenhagen.

LINDA FREY
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WOLFGANG SCHIEDER, editor. *Liberalismus in der Gesellschaft des deutschen Vormärz*. (Geschichte und Gesellschaft, Zeitschrift für Historische Sozialwissenschaft, number 9.) Göttingen: Vandenhoeck und Ruprecht. 1983. Pp. 362. DM 58.

On May 27, 1832, between twenty and thirty thousand Germans gathered in the town of Neustadt and marched, with flags of black, red, and gold and a banner proclaiming "Germany's Rebirth," to the castle ruins near Hambach. This meeting, thereafter known as the Hambacher Fest, was German liberalism's first great popular demonstration and a clear indication that the age of participatory politics had dawned in Central Europe. For obvious reasons, the fest's centennial, which fell in the dark days of Weimar's final agony, was not much noted, but in May 1982, with a liberal republic firmly established in West Germany, its 150th anniversary was marked by celebrations at the Hambach site, a television special, and a scholarly conference convened under the chairmanship of Wolfgang Schieder at the University of Trier. *Liberalismus in der Gesellschaft des deutschen Vormärz* contains the papers delivered at this conference, many of them in revised and emended form.

The book's title suggests the conference's central theme, which Schieder, in his thoughtful introduction, calls "the question of German liberalism's social

anchorage" (p. 10). The concern for ideas, foreign policy, and political biography, which dominated the historiography of German liberalism for so long, has clearly given way to an interest in the social origins of the movement's leaders, the strength and character of its institutions, and the depth of its support in the social structure.

The fifteen contributors to this volume approach these issues from several different directions. In their essays on Georg Friedrich Kolb and David Hansemann, Heiner Hann and Kurt Düwell use a biographical mode to pursue questions of economic theory and practice. Rudolf von Thadden and Frank Eyck examine the centrally important but too frequently underestimated role of religious issues and institutions in the emergence of the liberal opposition. Particular economic issues and the political behavior of specific social groups provide the focus for Hans Werner Hahn's contribution on the *Zollverein*, Hans-Ulrich Thamer's on liberals and artisans, and Karl Wegert's on the role of popular political traditions in liberal agitation. Günter Birtsch pursues his long-term interest in constitutional and legal affairs with an essay on liberals and the problem of basic rights, while Cornelia Foerster summarizes some of the research published in her recent monograph on the *Press- und Vaterlandsverein*. Rudolf Muhs and Elisabeth Fehrenbach add to our knowledge of regional politics with discussions of Saxony and the Rhineland. Rudolf Vierhaus and Innocenzo Cervelli deal with liberal elites, the former by discussing the relationship of liberals to the bureaucracy, the latter by analyzing the composition of some important liberal organizations. Finally, there are two comparative essays: Nicholas Hope's on liberalism and the churches in Great Britain and Scandinavia and Dieter Langewiesche's stimulating reflections on French and German liberalism in the revolution of 1848.

Taken together, the essays show us a complex and multifaceted movement that drew on a variety of support, exploited diverse sources of discontent, and mobilized many different social groups. Among liberalism's followers were to be found Catholics and Protestants, free traders and protectionists, entrepreneurs and artisans, radical agitators and well-placed bureaucrats. "The unity of the liberal movement in Germany," Schieder notes, "came only from its diversity" (p. 21). I would be inclined to phrase this somewhat differently: the liberal movement's efforts to unite were, it seems to me, ultimately defeated by the diversity of its components.

As is always the case when reviewing collections of this sort, an evaluation of each contribution is impossible. Suffice to say that the overall quality of these essays is very high indeed. Many of them present important new research and interesting new ideas. Almost all of them advance the enterprise as

defined in the editor's introduction. Schieder, therefore, deserves our admiration and gratitude.

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VOLKER PRESS, editor. *Städtewesen und Merkantilismus in Mitteleuropa*. (Städteforschung, series A, Darstellungen, number 14.) Cologne: Böhlau. 1983. Pp. xii, 333.

This collection of papers was prepared initially for a conference on "cities and mercantilism in Central Europe" at the Institute for Comparative Urban History at the University of Münster. Its editor, the leading historian of the German nobility in the sixteenth century, shows a keen awareness of the research problems concerning the relationship between the city and the early modern state. Had all contributors grasped Volker Press's analytical perspectives in his intelligent introductory essay, this book might have assumed greater significance in German urban history. As it stands, however, the individual articles move in somewhat different directions and display rather uneven quality. Some take seriously an attempt to analyze connections between urban developments and the economic policies termed mercantilism or, in the German states, more appropriately called cameralism. Several authors have chosen to discuss aspects of city history during the seventeenth and eighteenth centuries, however, without much concern for substantive links between cities and mercantilist or cameralist policies aimed at forging strong territorial economies and governments. One essay, Jürgen Weitzel's examination of legal thought and, especially, the influence of orderly "police" on the development of more dynamic notions of lawmaking, seems to have escaped the original organizational framework altogether.

A shift in this volume from the prevailing emphasis in German research on imperial cities to closer consideration of territorial towns is both expected and most welcome. Only two unimpressive essays deal primarily with *Reichsstädte*. Hans Mauersberg summarizes his already published analysis of competing systems of currency exchanges in the empire, and Wolfgang Klötzer defends Frankfurt from the "odium" of being opposed to industrial innovation. Klötzer fills in some details but gives no cause for a change of general interpretation in his unsophisticated argument, which focuses on some voices in favor of innovation within governmental circles rather than on the difficult struggle within the whole community between two profoundly opposed conceptions of industry and the economy. More important are the seven remaining pieces devoted chiefly to territorial cities, but they vary considera-

bly—from some disappointingly familiar and general remarks by Walther Hubatsch, through Karl Gutkas's straightforward but useful descriptive survey of Austrian urban developments, or Margret Zumstrull's long but suggestive article on Huguenot settlements in Kassel and Karlshafen as case studies of success and failure in territorial mercantilism, to some quite penetrating and important essays that deserve special attention.

Gerd Heinrich and Danuta Molenda present a striking contrast in their interpretations of urban history east of the Elbe. Molenda's short discussion of seventeenth-century mining towns in Upper Silesia and southern Poland, where powerful landowners developed economic practices that Polish historians now call "noble or magnate mercantilism," reinforces the conventional picture of eastern cities sapped of their industrial, commercial, and political potential by rural interests. This analysis brings into even sharper relief Heinrich's revisionist arguments concerning eighteenth-century Prussian cameralism, which fostered rather than discouraged urban growth. Through direct subventions, encouragement immigration, modernization of city services, and active capital investment in urban manufacture, Prussia maintained a higher ratio of urban-to-rural population (26:74 percent in the central and eastern provinces) than is realized by historians attached to the older view of "late feudal urban misery" in Eastern Europe. Prussian rulers and bureaucrats achieved relatively high productivity in manufacture, shifted economic resources from the agricultural to the urban sector, and, in general, forged the growth ethic characteristic of modern "bourgeois" economies. In addition to this thought-provoking essay and Press's introduction, the outstanding contributions to the whole collection are Richard Dietrich's article on Electoral Saxon cities and Wolfgang Leiser's comparative study of towns in several small south German states. With penetrating intelligence these last two scholars survey urban development in their regions, demonstrate the interaction of economic with political, constitutional, and social issues, and offer nuanced generalizations that do full justice to historical complexity in Central Europe. Although I cannot summarize their substantive conclusions here, I recommend these excellent articles not only to German specialists but also to all students of urban history.

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WALTRAUD SPERLICH. *Journalist mit Mandat: Sozialdemokratische Reichstagsabgeordnete und ihre Arbeit in der Parteipresse, 1867 bis 1918*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien,

number 72.) Düsseldorf: Droste. 1983. Pp. 263. DM 48.

Journalist mit Mandat is written by a journalist with a doctorate in "communication science." More accurately put, it is a practicing journalist's dissertation. It consists of some 140 pages of straight journalistic narrative, 80 pages of biographical appendix, and 20 pages of bibliography. There is almost no analytical framework, no effort at critically reviewing the literature or, for that matter, at formulating a political critique. A few remarks make it likely that Waltraud Sperlich's sympathies lie with "basis democracy," but more noticeable is a sense of cynical, journalistic bemusement. On balance, the author's portrayal of the Social Democratic journalists and Reichstag deputies is not flattering. The main outline of the story is familiar: the Social Democratic party was created by political organizers who were agitators in word and print. For decades Wilhelm Liebknecht's motto "knowledge is power" served to define a merely propagandistic role for the Social Democratic press, in spite of the widespread recognition that the press failed to win over many converts. It did not even appeal to most Social Democratic voters and addressed itself mainly to party members. It often failed to provide them with up-to-date information on current events. It could not compete with the "bourgeois" press in information and entertainment ("Feuilleton"). The great exception was the *Leipziger Volkszeitung*, which the embattled Bruno Schoenlank turned into an innovative, competitive journal after 1894.

About two-thirds of Sperlich's text describes the workings of the press and the reasons for its poor performance. Most journalists came from the working class, were badly educated, overburdened with many tasks (including regular speechmaking), and underpaid. Many were simply drafted by the party with the rationale that every activist who once wrote a letter to a paper could also edit one. The minority of journalists with bourgeois and academic backgrounds also were overworked and often had equal trouble eking out a living. On one side they were discriminated against by the bourgeois establishment; on the other they were forbidden by the party to write for bourgeois journals, even on nonpolitical matters such as literature and the arts. With considerable success the party leadership, especially August Bebel, maintained strict control over the press and contained intraparty polemics, even though Friedrich Engels occasionally prodded Bebel and Liebknecht to be less "Prussian," to permit more criticism and to let Social Democrats write for the very best bourgeois journals. When local party commissions representing the "party basis" were set up to balance the national headquarter's control, the journalists had to serve two masters. For journalists, there was no "freedom of the press." Matters got

worse during the First World War, when the Social Democratic movement began to split and the high level of personal conflict was exacerbated by irreconcilable ideological disagreement.

Only the last third of the book addresses the title topic, the relation of Reichstag deputies to the press. Although more than one-half of the 215 Reichstag deputies between 1867 and 1918 were "journalists with a mandate," a differentiation gradually emerged in the 1890s between those who became parliamentary pros and those who remained primarily journalists. Sperlich's two-page "Schlusswort" does not amount to a proper conclusion, but, as an assemblage of data, this dissertation may have its limited use.

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RICHARD CHARLES MURPHY. *Guestworkers in the German Reich: A Polish Community in Wilhelmian Germany*. (East European Monographs, number 143.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1983. Pp. xi, 255. \$22.50.

The immense and rapid expansion of the coal-mining area in the Ruhr after 1870 attracted huge numbers of migrants, among them, mainly after 1890, thousands of Poles from the eastern Polish parts of Prussia. These Poles formed their own communities in the towns of the Ruhr region, maintaining their native language and founding their own associations. After World War I some of them migrated to the newly established Polish state; others went to the French coal-mining area. Most, however, stayed and eventually became completely integrated into German society.

The book under review is an urban case study about the development and circumstances of the Polish immigration to, residence in, and partial emigration from Bottrop, a town in the Ruhr region, between 1890 and 1933. On a two-year visit to Germany Richard Charles Murphy extracted valuable material from the municipal archives of Bottrop and the Staatsarchiv Münster. His main sources are the records of the city clerk's office and the registration office (*Standes-/Einwohnermeldeamt*). Out of more than 200,000 registration forms he took a random sample of 322 persons and a sample of these immigrants' 311 children. His arguments are supported by a great number of tables, which, tabulated with the aid of a computer, present the figures drawn from the sample. After a short draft of the historical development of the *Ruhrgebiet* and of Bottrop, the author traces the immigration and emigration and describes the economic structure of the region; the occupational mobility of the sample

group; working conditions; the activities of the Polish Trade Union; the impact of religion, of education in school and at home; connubium, housing, and residential patterns; and political, organizational, and voting behavior.

An American's special interest in the subject may stem from the concept of the "melting pot," which the *Ruhrgebiet* is commonly considered to be. Murphy is mainly interested in discovering and explicating the means and extent of the social accommodation of the Poles into the structure of the *Ruhrgebiet* (p. 3). To the melting pot concept he opposes a concept of cultural pluralism, which he makes plausible with support from many findings. The maintenance of a Polish cultural community was facilitated by the large number of Poles, by the homogeneity of the language and the religion of the immigrants, by the qualifications that they brought from their native areas that helped them obtain good earnings, and by the urban situation of Bottrop, which was a town expanding at a tremendous rate. The German pressure for assimilation (not only in the schools) remarkable economic success, the absence of economic discrimination at the mines (p. 193), and the residential structure at Bottrop (where there were areas predominantly inhabited by Poles but no ghettos) strongly aided social integration.

Yet the title of the book is irritating. The Poles in question were neither politically nor residentially in the position of "guestworkers," and the book covers the period up to 1933, not just "Wilhelman Germany," as the title indicates. The findings concerning Bottrop are always combined with information (sometimes too much information) about the situation (for example, of housing) in the *Ruhrgebiet* as a whole. The book, however, owes more to the author's statistical research than to existing literature. Material from business archives probably could have widened the information about working conditions.

The main conclusions of the book are not completely new, as the author himself admits (pp. 6, 189). Yet, undoubtedly, one of the book's merits is that it examines what is already known through a subtle empirical analysis of mass data. Although the categories of the analysis could be argued about in detail, the book brings up a number of implications and adds both greater precision and new details to our knowledge of the topic. This is true not only of the scrupulous inquiry into fluctuation, which in an urban population constantly in flux, showed an astonishingly large number of long-term Polish residents (more than ten years). But it is also true, for instance, of some still widely unknown factors such as connubium (mainly within the Polish community and the faith and strata boundaries) and intergenerational social mobility.

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ULRICH HEINEMANN. *Die verdrängte Niederlage: Politische Öffentlichkeit und Kriegsschuldfrage in der Weimarer Republik*. (Kritische Studien zur Geschichtswissenschaft, number 59.) Göttingen: Vandenhoeck und Ruprecht. 1983. Pp. 362. DM 78.

In the 1920s and 1930s (and again in West Germany in the 1960s), the controversy over the origins of World War I raised issues not only of scholarship but also of current politics and of the preservation or overthrow of currently accepted beliefs. Living people stood to gain or lose. In such cases, the controversies themselves become worthwhile objects for historical study.

Ulrich Heinemann has examined in detail the Weimar Republic's campaign against war guilt and has drawn some fascinating conclusions about the campaign's implications for Weimar politics. He accepts the view that the principal official "foundation" of war guilt, Article 231 of the Versailles Treaty, actually did little more than provide the legal premise for a reparations liability. But leading Germans *wanted* to believe that the whole treaty rested on an assumption of German guilt. They were confident that, with official documents, they could conclusively refute the war-guilt charge and thereby show the treaty to be a miscarriage of justice. And the issue would also reunify the country.

Building on earlier findings by Imanuel Geiss, Heinemann depicts Bernhard von Bülow, later the chief permanent official in the foreign ministry, as the leading architect of the organized anti-war-guilt campaign. Bülow wished to influence foreign opinion and to develop a people's movement (*Volksbewegung*) within Germany to prevent the world (as he put it) "from forgetting the injustice done to us, and from believing that Germany has come to accept the injustice" (p. 57). He and others at first wanted a private organization, but backing proved hard to find, and the foreign ministry eventually resorted to covert subsidies, using two ostensibly private groups: the Zentralstelle für Erforschung der Kriegsursachen encouraged foreign revisionism, while the Arbeitsausschuss Deutscher Verbände, an umbrella organization, helped to fund and to oversee and control the numerous (usually conservative) domestic organizations interested in the war-guilt issue. Until 1930, this domestic control succeeded surprisingly well. Heinemann believes that the campaign abroad was ineffective, but this reviewer would disagree, the Zentralstelle's periodicals and contacts did provide support to British and American revisionists, as did the foreign ministry's documentary publications, and revisionist views became more influential in London and Washington than Heinemann's almost entirely German sources show. By present-day standards, the costs of the two programs were modest.

Heinemann suggests that the subcommittees of the Reichstag's investigatory committee offered a chance, especially to socialists, to bring to light the responsibility of imperial leaders, not only for starting the war but also for violating the rules of war, causing unnecessary economic hardships to enemy peoples, failing to negotiate a peace, and contributing to defeat. But he recognizes the strength of authoritarian traditions; for example, Reichstag deputies almost always deferred to the superior wisdom of the foreign ministry.

From the German Nationalists through much of the Social Democratic party, German opinion—unsurprisingly—was governed by ideas from the past, including slogans of wartime unity. In 1919, Allied opinion was also much influenced by wartime convictions, as the Germans soon discovered. When hopes of an easy "Wilson peace" were dashed, the republican middle-class leaders scurried for nationalist cover. Heinemann notes that the war-guilt campaign strengthened, if anything, prewar tendencies toward a rejection of uncomfortable foreign realities and toward a national autism, in the sense used by Dieter Senghaas. (One could also cite Theodore M. Newcomb and Karl W. Deutsch.) In the early 1930s, the campaign would serve the interests of the antirepublican right. The republican revisionists eventually found themselves, as Heinemann aptly concludes, in the role of the sorcerer's apprentice.

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RAYMOND L. PROCTOR. *Hitler's Luftwaffe in the Spanish Civil War*. (Contributions in Military History, number 35.) Westport, Conn.: Greenwood. 1983. Pp. x, 289. \$29.95.

The question of the timing, quantity, role, and significance of foreign military involvement in the Spanish Civil War has been much debated. This book, based chiefly on Luftwaffe archives and the author's interviews with German officers who served in Spain, looks at one aspect of that question, the famous Condor Legion. Raymond L. Proctor attempts little in the way of comparative analysis with the other actors in the air war and seems unconcerned to make any particular point about the significance of the Condor Legion's role in the war in general. His is the more limited "unit history" approach. Within these boundaries he provides a valuable record of the legion's origins and activities.

Many accounts of the Spanish war state or imply that German intervention, especially in the air, was rapid, massive, and decisive and that, consequently, the nationalists enjoyed air supremacy throughout. This view is not borne out by Proctor's work.

Rather, he says, it was hesitant and limited. Not until late October 1936, after Soviet Polikarpov I-15 fighters appeared in Spain, did the Germans decide to commit a combat air unit. Even then their best fighter, the bi-wing He-51, was hopelessly outclassed by the Russians. Only in 1937 did the first experimental models of the Me-109 arrive. And the nationalists did not gain air superiority until mid-1938.

The Spanish war did serve the Germans as a valuable proving ground for men, machines, and tactics. Among the most important lessons they learned was how to use air power for close support of ground operations. Tactics developed in Spain became standard and paid high dividends in the blitzkrieg campaigns of World War II. A second important tactical innovation developed was an effective combat formation for fighters, the so-called "Finger Four," later adopted by the RAF and USAF. On the negative side, the emphasis on the tactical uses of airpower to the virtual exclusion of the strategic in the Spanish war largely accounted for Germany's failure to ever develop an effective strategic air arm and cost them dearly in World War II.

Throughout the war some nineteen thousand Germans served a tour with the Legion. The average number of personnel at any given time was about five thousand and the average number of planes about one hundred. Legion flyers shot down 313 enemy aircraft and lost 72 of their own.

On the emotionally loaded question of Guernica, Proctor says only that the archival material available indicates that it was just one of many targets hit in the course of the Basque campaign and no special significance was attached to it by the Germans at the time.

The book suffers from a cumbersome and overly loquacious prose style and an organizational scheme that leads to too much space being devoted to ground operations that have been done before and add nothing of value. It will, however, be a valuable source for future workers in the fields of the Spanish Civil War, the Luftwaffe, and the development of air power in general, and is thus a welcome contribution.

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HORST BOOG *et al.*, *Das Deutsche Reich und der Zweite Weltkrieg*. Volume 4, *Der Angriff auf die Sowjetunion*; supplement to volume 4, [Maps]. (Beiträge zur Militär- und Kriegsgeschichte.) Stuttgart: Deutsche. 1983. Pp. xix, 1172; 27. DM 58.

This magisterial volume should leave no doubt that the world's most distinguished company of military historians is to be found in the *Militärgeschichtliches*

Forschungsamt. This agency, charged with producing the Federal Republic's history of Germany in the Second World War, opted for the challenging approach of assigning each volume to a team of scholars who prepare individual sections then coordinate their contributions. The result in this case is a complex work, whose authors sometimes put as much energy into debating each other as in seeking a common conclusion. It is also a comprehensive and successful synthesis of ideological and operational factors, the diplomatic, economic, and strategic elements, that contributed to the Operation Barbarossa's genesis and failure.

Hitler's attack on the Soviet Union ultimately may have been determined by his racially and ideologically based hostility to Slavic Bolshevism. Jürgen Förster demonstrates that it was also conditioned by the Wehrmacht's inability to defeat Britain, and by the speed of France's collapse in 1940. Barbarossa was not a gambler's gambit undertaken in desperation. It was, rather, an attempt to lead from strength. A lightning victory against a vulnerable major adversary would decide the war in Germany's favor and an attack on the Soviet Union seemed far more promising than any other feasible strategic alternatives. If France, the principal foe in German interwar planning, had been so quickly destroyed; the Soviet empire, with its primitive economy and clumsy, purge-weakened army, could hardly expect to survive. German planners were well aware of the Soviet Union's potential strength. They expected, however, to conquer before that strength could be mobilized.

Joachim Hoffmann functions as the work's Greek chorus, presenting the war from a Soviet perspective. He successfully dissipates any lingering images of the Soviet Union as a well-intentioned victim of Nazi aggression. In general terms, the Soviet Union's immediate and remote approaches to her European neighbors were malevolent enough to give significant credibility to Hitler's argument that Germany was waging a legitimate preventive war. Specifically, the Soviet Union cooperated with the Nazi regime until the invasion itself and avoided disaster less by virtue of her own positive qualities than because of her enemy's compounded mistakes of judgment and execution.

Förster and Rolf-Dieter Müller combine to develop a line of argument more familiar in East German and Soviet historiography than in the West, stressing the essentially colonial nature of Nazi Germany's intentions toward the Soviet Union. The Wehrmacht was expected not only to supply itself completely from the occupied territories but also to develop them as a source of labor, food, and raw materials for an overstrained German economy. This meant ruthless, systematic exploitation of the conquered people. Arguably the strongest, certainly the most impassioned, sections of this work are

those in which Förster exposes Wehrmacht complicity in and support for Hitler's genocidal policies. Far from holding themselves aloof, generals and privates alike accepted and acted on Nazi proclamations of a war of "conquest and annihilation," translating ideology and propaganda into military orders with few discernible qualms. Stalin's own determination to wage total war may have exacerbated the process, but the Germans began it, with blind unconcern for possible results.

A similar insouciance characterized every aspect of German planning for Barbarossa. Ernst Klink's analysis of the military preparations is a case study in misdirected vitalism. The army that invaded the Soviet Union was a patchwork of improvisations, equipped with vehicles and weapons from all over Europe, with supply and replacement programs based more on wishful thinking than on sober calculation. Intelligence was similarly improvised and incomplete. Warnings that Russia had changed drastically since 1918 were lost in a general surge of optimism—an attitude encouraged at least in part by a reluctance even to consider the consequences of anything going wrong. Nor were diplomacy and propaganda able to generate support for a European crusade against Bolshevism. Gerd Ueberschar for Scandinavia and Förster for southeastern Europe demonstrate Hitler's failure to develop reliable alliances. Romania and Slovakia, Finland and Hungary, supported war with Russia more or less enthusiastically—but for their own reasons. From the beginning they proved to be almost more liabilities than assets to the German effort.

In this interpretive context it is hardly surprising that the campaign itself emerges as an anticlimax. Operational narrative, primarily at army level, takes up less than one-third of the book and argues that from the beginning the Wehrmacht was victimized by its own excessive expectations. The great early victories did not destroy the Soviet army. The great early advances left too much of the Soviet state unswallowed. The campaign had no real turning point. Its course reflected instead the logical consequences of inadequate preparation and visionary planning. By the end of August even Hitler saw that victory in 1941 was impossible. Yet the generals continued attempting to force a decision, as a stumbling man seeks to restore his balance by accelerating his pace. This was the real beginning of that mental retreat into battle-fighting so characteristic of German leadership on the Russian front. But short-range triumphs could not compensate for basic failures of strategic vision and grip—a point clearly made by Horst Boog. His treatment of the Luftwaffe's role highlights the way an air force used to supplement the army's deficiencies became so directly involved in the land battle that it lost whatever independent vision it once possessed.

Nor were the Germans any better at exploiting what they did conquer. Müller presents a black comedy of errors, of overlapping jurisdictions and contradictory programs, of nothing going right because nothing was properly planned. The result was a policy of diagonals, chiefly remarkable for waste and inefficiency at all levels. If the Third Reich's goals harked back to the Thirty Years' War, the Nazis never produced a Wallenstein able to make the occupation more than a gigantic plundering raid. Whether behind the lines or on the battlefield, Operation Barbarossa stands as grisly testimony to the necessity of harmonizing reach and grasp, will and means in national policy.

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FRANCESCO BENIGNO. *Il porto di Trapani nel Settecento: Rotte, traffici, esportazioni, 1674–1800*. Foreword by G. GIARRIZZO. Trapani: The Author. 1982. Pp. 177.

This work represents a fine opportunity missed. Francesco Benigno gained access to a private archive in Trapani and to an eighty-one-volume series from the daily records of that city's harbor customs officials (*credenzieri*) from 1674 to 1800. Trapani, of course, was only a modest center, overshadowed by its larger neighbor, Palermo. But like many other middling or small harbor towns in the Mediterranean, it was the hub of short-range coastal trade, and it came to be tightly linked to the wider tempos of the Mediterranean and the world economy. A proper study of the Trapani harbor movement, then, could have been instructive on several planes of time and space, of economy and society. Largely because of conceptual problems, however, Benigno has produced a study that is narrow in focus, disjointed in form, and restricted in scope.

If a thesis can be deduced from Benigno's account, it is that Trapani underwent a process of deep transformation that saw its old role as a transit port on the Levant and North Africa route changed to that of an export harbor, an outlet for local products. Especially in the later eighteenth century, the export of salt, finished coral, and soda ash grew so as to compensate for the decline of older exports and to assign Trapani a "stable role" in Mediterranean trade. (p. 104).

Benigno, then, is tracing a process of dislocation and change, of adaptation and recovery. But very little sense of that process emerges from the pages of this work, largely because Benigno is concerned primarily with accounting for change only over the very short term. The reader is confronted by pages of garbled narrative that seek to explain the many

peaks and troughs in the tables appended to the work. The author flounders from one dip to another, episodically describing each "crisis" it allegedly represents and ascribing them all to a single factor—war, from the 'Thirty Years' War to the French revolutionary wars. There is no doubt that war makes for dislocation and disruption, and that an export economy like Trapani's, dependent on foreign demand, would be susceptible to its vagaries. But, to determine its effect on Trapani's export trade, Benigno should have done better than to peg rigidly the city's harbor movement to the vicissitudes of international diplomacy or to appeal to the pronouncements of a mid-nineteenth-century liberal writer, G. E. Di Blasi. Benigno's explanations are monocausal and mechanistic, and they contribute to the strangely musty flavor of this work. By assigning a priori predominance to high diplomacy, Benigno has chosen to neglect or to obscure those indigenous factors that made for the remarkable adaptation or even the spectacular growth of some sectors of the economy. What little in this work touches on the internal dynamics of Trapani (or even its relation to Palermo) is crude and unsophisticated.

The strength of Benigno's work lies only in the tables it presents (limited, though, to exports). This is rich data indeed, a reflection on the sources themselves. In more expert hands they could have yielded an important study, as analogous (import) data did with Fernand Braudel and Ruggiero Romano's classic *Nauvres et marchandises à l'entrée du port de Livourne 1547–1611* (1951).

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GEOFFREY SYMCOX. *Victor Amadeus II: Absolutism in the Savoyard State, 1675–1730*. (Men in Office.) Berkeley and Los Angeles: University of California Press. 1983. Pp. 272. \$27.50.

This book constitutes an important contribution to the historiography of early modern Europe for two chief reasons. First, it stands almost alone in the English language literature in providing information and insights into the foreign policy imperatives and, even more important, the internal political and economic development that permitted Savoy alone among the Italian states to play a persistently significant role on the European scene as a second-class power in the seventeenth and eighteenth centuries. Second, it not only provides an excellent case study in the development of absolutism, but it also links that development to the increasingly efficient long-term organization of internal resources that pointed

(with some unevenness along the way, to be sure) toward the later unification of Italy by a Piedmont-Sardinia to which centralistic and bureaucratic traditions rooted in the early modern period gave the necessary strength and determination for its historic role.

Although the work is presented as a biography of Victor Amadeus II, the personality and personal history of this remarkable duke retreat behind the much fuller and more informative account of his foreign and domestic policies. The sketchy treatment accorded the man is due partly to a lack of documentation stemming from the duke's own secretiveness and dissimulation—common in an age when laws were less than men and personalities were still more important than institutions—and partly to the unreliability or bias of second-hand reports. Moreover, Geoffrey Symcox's real intention is to give his readers a general description of the evolving nature of the Savoyard-Piedmontese state; and although the role of Victor Amadeus in this evolution was of great importance, his reign and policies become, in one sense, only a vehicle for this larger purpose. The author is careful to point out both the existing traditions on which Victor Amadeus built and the traditional mind-set of the duke himself, which made his reforms, extensive and important as they were, more reminiscent of seventeenth-century (and largely political-administrative) state building than of the conceptually more innovative reforms associated with eighteenth-century "enlightened despotism."

Although the progress of absolutism in Savoy had many features peculiar to itself, features which will be noted with interest by historians with knowledge of only the larger or more extensively treated states of the period, a comparison of Savoy's development with that of Brandenburg-Prussia is almost unavoidable—and, in fact, is solicited by the author at several points. Special geographical problems, emphasis on establishment of a military force much larger in proportion to population than that of other states, and the obsession with the elimination of traditional secondary authorities concomitantly with the establishment of an all-powerful crown overseeing a tightly organized bureaucratic hierarchy—these are but a few of the similarities that impress. The apparently opportunistic foreign policy of both states is equally striking, and here Symcox shows a shrewd understanding of the special problems of a second-class power caught between the machinations of the mighty. Too proud to accept a permanent status of clientage, and with neutrality seldom a real choice, Savoy survived—and even grew—by policies better described as realistic than as opportunistic.

The author describes his own work as one of synthesis, inasmuch as he has relied very largely on

secondary rather than primary sources for his information. But those secondary sources are extensive and well chosen, and what emerges as a final result—assisted by a pleasing style—is a useful and important book. And if indeed one could wish greater detail in some aspects of this history than appear here, one cannot but be grateful for this contribution to an area of European history of which too many of us remain embarrassingly ignorant.

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Mazzini: Tra insegnamento e ricerca. (Atti del Seminario di Aggiornamento, 1981; Risorgimento: Idee e Realtà, new series, number 3.) Rome: Edizioni dell'Ateneo, for the Istituto per la Storia del Risorgimento Italiano. 1982. Pp. 179. L. 10,000.

NARCISO NADA. *Guglielmo Moffa di Lisio, 1791–1877: Il contributo di un patriota braidese al Risorgimento nazionale.* Bra: Società Amici del Museo. 1982. Pp. xii, 157.

Local and regional Italian historical societies continue to help publish studies of Risorgimento figures, as the friends of Bra's museum have done in the case of Narciso Nada's biography of Guglielmo Moffa di Lisio. In 1981 the Istituto per la Storia del Risorgimento in Rome participated in a conference to address the state of current Mazzinian scholarship. The conference also considered the infusion of recent work in that area into the textbooks of secondary schools as well as the roles of such historical societies and museums in promoting the teaching of history at a time when historic figures compete with the popular heroes of the space age.

Nada's task in undertaking a biography of Lisio, a Piedmontese revolutionary in 1821, was not an easy one, since his subject left no diary, autobiography, or great repository of letters. Thus his materials had to be gleaned from those of Lisio's more famous Piedmontese relatives and friends, the Alfieri and d'Azeglio families, or French friends met in exile, such as Victor Cousin. Lisio is a comparatively minor figure in Risorgimento history, seemingly the victim of circumstances, not the least of which resulted from French occupation of Piedmont which led to a career in the Napoleonic army and service in Spain following education at St. Cyr. He was also part of a generation infused with the literary liberalism of Ugo Foscolo and Vittorio Alfieri. Commissioned in the Piedmontese army after 1815, he joined a secret society, the *Federati*, actively participated with fellow officers in planning the revolution of 1821, and earned an eleven-year exile in France. Fully pardoned only in 1843, his low profile and cautious politics identified him with

right-wing liberals and led to the portfolio of war in 1848, involvement in Charles Albert's campaign against Austria in 1849, and the role of deputy representing Bra in the Subalpine parliament from 1850 to 1860 (years in which he made but one speech, in 1853).

One might question whether Lisio rates a biographical study, but despite the dearth of documentation Nada has skillfully produced a portrait that demonstrates the personal toll often levied on revolutionaries: Lisio's activities kept him from any military and political career for twenty-two years and inhibited his later political involvement (and perhaps a personal one, since apparently he never married), although his 1821 goal of a constitutional monarchy was realized in Piedmont in 1848. Used as a sounding board by others like Cousin (reflecting French views on Italian unity) or as a consultant by politicians like Cavour (on the 1847 *statuto*), Lisio's moderate liberal views seem best expressed through his well-known correspondents.

Those who research Giuseppe Mazzini do not lack materials, as Vincenzo Pacifici illustrates in discussing available editions of the revolutionary's works in the proceedings of the 1981 conference. Eminent scholars, like Emilia Morelli, suggest areas, such as the wider European revolutionary movement and Mazzini's connections with other trade union movements, in which work remains to be done despite the wave of Marxist scholarship (examined by Franco Della Peruta) on Mazzini and Italian revolutionary and working-class movements since the 1950s. Research still to be undertaken in the broader world context is further illustrated by the Japanese historian of the Risorgimento, Fusatoshi Fujisawa. The historical detection of Fujisawa traces a network connecting Mazzini's influence through an American-based Social Gospel group in Meiji Japan in the 1890s and the Japanese socialist Tomoyoshi Murai.

The conference's attention to teaching, textbooks, and historical organizations mirrors a similar interest in other countries (as exemplified by the American Historical Association). Alberto Arpino examines the role of historical institutions, such as museums, in cooperating with and providing programs to teachers in the secondary schools, and Elena Piazza reviews history texts that have been defascitized and, in part, denationalized since 1945. Piazza sees the textbook as a hybrid between an encyclopedia of facts and a critical monograph, incapable of making Mazzini real unless the student is personally involved through an interdisciplinary research project. Although a "state of current studies" publication, while useful and necessary, is itself a hybrid between a catalogue of work accomplished and critical scholarship; the quality of the participants and the consideration given by the 1981

conference to such questions of instruction distinguish the present volume.

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GUIDO DONNINI. *L'Accordo italo-russo di Racconigi*. (Quaderni della Rivista *Il Politico*, number 19.) Milan: A. Giuffrè, for the Facoltà di Scienze Politiche dell'Università di Pavia. 1983. Pp. ix, 304.

The political science faculty of the University of Pavia, editors of *Il Politico*, have published number 19 of their timely *quaderni*, or booklets. The author, Guido Donnini, has drawn from the archives of foreign ministries in Rome and Paris, from the British Public Record Office, from published diplomatic documents of France, Italy, and Austria-Hungary, but not from Russian records in Soviet archives. A footnote spanning pages 199–201 has a bibliography of mostly older, general works. Carlo Morandi's *Le relazioni tra l'Italia e la Russia dal 1900 al 1917* (1949) is not mentioned; neither is *Rossia i Italia* (1968), edited by the Soviet History Institute of the Academy of Sciences. Nonetheless, the volume has such rich documentary quotations that it may be considered an archival font in its own right.

Prosaic style and format tend to conceal an *opéra bouffe* story. Italy was smarting from the fact that the tsar had not returned Victor Emmanuel III's visit to St. Petersburg in 1903. Russia was hurt because Italian opinion had favored Japan in the Russo-Japanese War. Then in 1905 Alexander Petrovich Iswolski's cousin, Nikolai Valerianovich Muraviev, became ambassador to Italy. He secured a commercial treaty in 1907 and, after Austrian annexation of Bosnia-Herzegovina on October 6, 1908, began actively to negotiate an imperial visit in the interest of another treaty. Suddenly, on December 14, 1908, Muraviev suffered a fatal heart attack and Iswolski's attitude became "incerta e dilatoria." The Italian foreign minister, Tommaso Tittoni, acting through Giuseppe Avarna di Gualtieri, Italian ambassador to Vienna, then began to negotiate with a reluctant Austria. Meanwhile, after months of tsarist indecision as to where, when, and how to visit the king, both Russia and Austria replied favorably on the same day, September 23, 1909. Tittoni played a double game.

Nicholas II visited Victor Emmanuel III at Racconigi on October 23–24, 1909, and clinched a treaty giving Russia a free hand in the Straits and Italy its way in Tripoli and Cyrenaica. Avarna was not apprised of the true nature of the accord and kept talking for a new triple alliance of Austria-Hungary, Russia, and Italy. Tittoni lied to Austria in saying that Racconigi was merely to preserve the

status quo in the Balkans, and he obfuscated matters still more by calling the new triple alliance proposal "purely academic." Only the dismissal of the Giovanni Giolitti cabinet on December 2, 1909, ended the anomaly. Foreign policy remained charged as Italy secured Tripoli through war with Turkey in 1912. Russia then revealed Racconigi to France but kept other interested capitals guessing.

Donnini surveyed foreign official and press reactions to Racconigi and rather abruptly ended the book with an index of names but with neither a summary nor additional bibliography. His documents show Racconigi as a secret deal in a setting of old-world hospitality, meshed in lies and playacting for effect and profit.

Another *quaderno* is needed to show how *opéra bouffe* became all too real *opéra guerre* and how the curtain came down on an age.

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JAROSLAV ŠIDAK. *Kroz pet stoljeća hrvatske povijesti* [Through Five Centuries of Croatian History]. Zagreb: Školska Knjiga. 1981. Pp. 377.

Students of history are once more indebted to Jaroslav Šidak, Croatia's most prominent historian, for a valuable contribution to Croatian historiography. His latest work is a collection of essays previously published in various journals and collective works. Probably no other historian knows Croatian historiography as well as Šidak, and this is why his essays are so important. Each essay is on a major topic from the history of Croatia, assessing the existing status of research on the subject, including the author's own insights. Apart from minor stylistic improvements, the essays are as originally published. The bibliographic notes in the footnotes, however, were brought up to date by the inclusion of the latest published works. The essay on "Croatian National Revival" was supplied with a bibliography, which it lacked when first published.

Šidak groups his essays under five major subjects. The first group of essays treats the major developments in Croatia in the sixteenth century. Two of them focus on the much-discussed peasant uprising in 1573, and one essay is on Nikola Šubić Zrinski ([ca. 1508–66] the "legendary defender of Sziget"). The second group of essays includes three on Juraj Križanić ([ca. 1618–83] writer, traveler to Russia, and defender of Vienna in 1683), one on the beginnings of Croatian political thought (which he starts with J. Križanić and P. Ritter Vitezović), and one on Petar Zrinski and F. F. Frankopan's conspiracy (1666–71). Particularly valuable are the essays on Juraj Križanić in Croatian and Serbian literature. The third group of essays includes two on education

in Croatia, one of them on the *Regia Scientiarum Academia*, founded in the latter part of the eighteenth century, and the other on the history of education in the Croatian lands (1773–1874). These two essays investigate the state of education in Croatia in the period preceding the Croatian national awakening, an important topic that is examined in the essays that comprise the fourth group. One essay in the fourth group discusses the Croatian national awakening in general, and the other three deal with specific topics, such as Ljudevit Gaj (the principal among the Croat awakeners), the attitude of the Turopolje “sandalized nobles” to the Illyrian movement, the role played by the city of Karlovac in the Croatian national awakening, and the Croatian movement at the time of the revolutions of 1848–49.

The fifth and last group of essays deals with four closely related topics. The first of these is on Ivan Kukuljević, the founder of modern Croatian historiography and a proponent of South Slav unity. The second essay is on a modern Croatian historian, Josip Matasović, and his writings on the Bogumils—the heretical “Church of Bosnia.” The third essay assesses the views of August Cesarec (the well-known Croatian writer) on Eugen Kvaternik and the Croatian Party of the Right in the 1871 crisis. The fourth essay in this group, and the last in the book, is on the history of Croatian historiography. In it Šidak surveys critically Croatian historiography from Ivan Lucius and his *De regno Dalmatiae et Croatiae libri sex* (Amsterdam, 1666) until 1971. Šidak contends that in terms of methodology and overall quality, Lucius’s work was on the level of *European historiography*. The tragedy was that for a long time after his work appeared there was no one in Croatia to continue in his footsteps, despite some isolated attempts to investigate Croatia’s past. Not until the end of the eighteenth century, Šidak writes, does one find another gifted historian, this time Josip Mikoczi ([1734–1800] a Jesuit and university instructor), who was able to emulate contemporary European historiography with his posthumously published work *Otiorum Croatiae liber unus* (Budaë, 1806). After Mikoczi it took another half-century before one again encounters a serious study of Croatian history. But as a result of the awakened national consciousness, the interest in national history grew steadily as Croatian historians, in resisting the aggressive Magyar nationalism, sought inspiration from their people’s medieval greatness and their centuries-old struggle for survival.

In his essay on Croatian historiography, Šidak gives his views on Illyrian historiography, the works of such prominent historians as Kukuljević, Franjo Rački, Tade Smičiklas, Ferdo Šišić, and others, and explains the stimulus to historical work created by the founding of the Yugoslav Academy of Sciences

and Arts in Zagreb. After a brief comment on Croatian historiography in the interwar period, Šidak looks at what has been accomplished since then. In this connection he lists leading historians, centers of research, and major publications. Unlike those Croatian historians who minimize the quality of the postwar Croatian historiography, Šidak lauds it. Although he alludes to a number of Croatian historical journals, Šidak, a modest man, does not say that he, more than anyone else, was responsible for making Croatia’s *Historijski zbornik* Yugoslavia’s finest historical journal.

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EDGAR ANDERSON. *Latvijas Vēsture, 1920–1940: Ārpolitika* [History of Latvia, 1920–40: Foreign Affairs]. Volume 1. Summary in English. Stockholm: Daugava. 1982. Pp. 703.

Thirty years ago in Sweden the Latvian-language publishing house Daugava began a multivolume series dealing with the history of the Latvian people from the medieval centuries to the present. Edgar Anderson’s work on the foreign policy of the Latvian state from 1920 to 1940—the first of a two-part volume—is the ninth contribution to the series. All of the volumes in the series are of the same magisterial scope, size, and thoroughness as Anderson’s work, but all, at the same time, are unlikely to have much of an impact beyond a relatively small group of Baltic area specialists because the entire series is written in Latvian. This seemingly deliberately chosen inaccessibility is, in a sense, an extension of the story that Anderson chronicles so well in the present work. The inability of Latvian foreign policy to alter the destructive impact on the Latvian state of large-nation power politics meant, among other things, that the period during which Latvian historiography began to develop became, for the Latvians, a kind of interregnum, with the Second World War bringing with it the emigration of a substantial proportion of Latvian historians to Western countries. Subsequently, historical writing in Latvian has been the product of historians in Western countries working at some remove from many—though by no means all—crucial primary sources, and of historians in Soviet Latvia writing in Latvian or Russian or both with immediate access to primary sources but with an eye to official ideology. One looks to the former group for the continuation of the tradition of unconstrained history of the interwar period, and this self-assigned task of continuing a tradition explains the use of the Latvian language in the series even though a number of its authors, especially Anderson, have also written widely in other Western languages.

Anderson's book is his second contribution to the Daugava series; his first, a 754-page work published in 1967, dealt with Latvian history in the years 1914–20 and covered all aspects of the founding of the Latvian state. In the present work, he takes up the thread of Latvian external relationships during independence and adroitly discusses Latvian foreign policy not only in terms of its substantive content but also with respect to the impact on it of the internal political changes the Latvian state underwent in the first twenty years of independence, of changing relationships among the great powers, and of the personalities of the diplomats (such as Z. A. Meierovics and Vilhelms Munters) who were in charge of its implementation. There was of course no question that after hundreds of years of subordination to German, Swedish, Polish, and Russian superiors, the Latvians had finally registered their presence on the European map in the form of a sovereign state with a parliamentary government. Latvian representatives now sat in the League of Nations and established embassies on the basis of at least formal equality with other states. Yet, as Anderson makes clear, the principle of self-determination meant more to the new small states than to the older large ones. Although Anderson does not strive to make the history of Latvian interwar diplomacy gloomy reading and seeks always to balance failures with accomplishments, a sense of doom and foregone conclusions suffuses the story that he has to tell. More goals—such as Baltic regional security—were thwarted than realized, in part because of the geopolitical vulnerability of the Baltic region and in part because of the unexpectedness of independence. As Anderson comments: "The possibility of greater gains in the world's forum for Latvia and the other Baltic states was reduced by lack of resources and experience, by insufficient information, and by diplomats who were generally speaking ill prepared for carrying out their tasks" (p. 387). The present work takes the story of Latvian diplomacy to about 1938, that is, to the years immediately following the establishment of the authoritarian regime of Karlis Ulmanis. The second part is to deal with the years from 1938 to 1940 and is to include a full bibliography.

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I. S. DOSTIAN *et al.*, editors. *Pervoe serbskoe vosstanie 1804–1813 gg. i Rossiia* [The First Serbian Revolt of 1804–13 and Russia]. Volume 2, 1808–1813. Moscow: Nauka. 1983. Pp. 396. 5 r. 40 k.

This two-volume collection of documents fills an important gap in Russian materials about the first Serbian insurrection and Serbian-Russian relations

from 1804 to 1813. Led by Karadjordje Petrović, the first insurrection inaugurated modern Serbian history and constituted the first significant Balkan national uprising against Ottoman rule. On its 175th anniversary, Soviet and Yugoslav scholars sponsored by the Soviet Academy of Sciences and the Serbian Academy of Sciences and Arts undertook this valuable enterprise. Until 1980 the Soviet editor was the distinguished Balkanist, S. A. Nikitin; after his death, I. S. Dostian assumed this task. The Yugoslav editor was the outstanding historian, Vaso Čubrilović, of the Serbian academy. These volumes (volume 1 covers 1804–07 and volume 2, 1808–13) contain over eight hundred pages of complete documents from Russian central and regional archives, especially the Archive of Foreign Policy (AVPR). Included are reports of Russian envoys in Serbia and Constantinople and correspondence with the Russian Foreign Ministry. The volumes include a brief introduction; photographs of Serbian and Russian leaders and excerpts from individual documents; and complete indexes of names, places, and foreign terms. Documents are in Russian, Serbian, and French with full Russian translations of French documents.

For the first insurrection, scholars must rely heavily on documents from foreign archives. Some native Serbian materials were destroyed in 1813, when Ottoman forces reconquered Serbia; others perished during subsequent invasions. Previously, documents have been published from archives in Vienna, Paris, and Zemun about the first insurrection (the most complete is Aleksa Ivić's multivolume collection in German from the Vienna archive). Before 1917 some Russian archival materials relating to this period were published and even more appeared in the Soviet period (especially *Vneshniaia politika Rossii XIX i nachalo XX veka*, vols. 1–5 [1960–67]). This, however, is the first Russian documentary collection devoted wholly to the first insurrection; it is comparable in format, significance, and excellence to *Osvobozhdenie Bolgarii ot turetskogo iga* (3 vols. [1961–67]), also edited by Nikitin.

The introduction to volume 1 supplies the familiar Soviet post–World War II interpretation of Russia vis-à-vis nineteenth-century Balkan national risings. In this "progressive struggle of oppressed peoples" against reactionary feudal Ottoman rule, tsarist Russia "played an objectively positive role" (1: 5). Whereas Austria and France spurned appeals of the Serbs for support and aid "Russia, giving consistent aid to Serbia during all the years of the insurrection, providing the insurgents with significant sums of money, weapons, and diplomatic support, conducted with them from 1806 to 1812 a common armed struggle against the Ottoman empire" (1: 7). Although the Ottomans finally crushed the first insurrection in 1813, while Russia was fighting

Napoleonic France, Article 8 of the Bucharest Treaty (May 1812), secured by Russian diplomacy, provided a basis for subsequent Russian support to Serbia's struggle for independence. Although the Soviet editors greatly exaggerate the extent and consistency of Russia's aid to the Serbian insurgents, by omitting documents that fail to support their preconceived views, scholars concerned with nineteenth-century Balkan history and Russo-Serbian relations will welcome this fine collection.

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LEWIS H. SIEGELBAUM. *The Politics of Industrial Mobilization in Russia, 1914–17: A Study of the War-Industries Committees*. New York: St. Martin's. 1983. Pp. xix, 312. \$27.50.

The end of tsarist Russia has attracted much research, and among the institutions discussed have been the War-Industries Committees (WICs). Lewis H. Siegelbaum—who wrote an Oxford dissertation on the WICs—has now published the first full-length study, a specialized volume full of material not only on the background to the revolution but also on the Russian middle class. The book begins with a discussion of the making of the Russian “industrial bourgeoisie”—important to the author's interpretation of the WICs as “essentially a nationalist movement” (p. 208)—and takes the story on to the bitter end of the WICs after October 1917.

The introduction evaluates some of the Western work on wartime Russia, including that by George Katkov, Norman Stone, and Raymond Pearson (although not Tsuyoshi Hasegawa, whose book evidently appeared too late); it is to be regretted that these sources could not also have been brought more into the main text, since there *are* disagreements. Siegelbaum's view is that the history of the WICs is more complex than often thought, and it is certainly true that he provides more detail than has previously been available; this comes partly from Soviet and Western archives and partly from rare published sources. Different business interests were involved (especially Moscow versus Petrograd), and rather than create a spirit of class peace—the *Burgfrieden*—the WICs helped to split even the Russian business community. Meanwhile, mutual suspicion meant that the WIC workers' groups did not have the shared interests with the industrialists that is assumed by some Western and Soviet writers. Also stressed is the hitherto-neglected “third element,” the technical intelligentsia, which held the WICs together once other tensions increased.

Siegelbaum makes a convincing case that, even if the WIC leaders did not achieve their own goal of

appropriating the administration of war industry, the committees were more successful than critics have allowed, at least in certain sectors of production and in applied technology. And he is also right in his conclusion that, in essence, the WICs failed. They never represented the private sector as a whole, and they were unable or unwilling to work with the state for the regulation of the economy—as the industrialists' organizations did in other warring countries. They “contributed to the prevailing chaos” (p. 158) and helped—accidentally—to bring about the downfall of the whole tsarist system.

Although there is a long discussion of the political side of the WICs, it is a pity that the description and analysis of their role in the February Revolution itself was not developed even more; after all, this is a major reason for interest in the WICs. The author does argue, however, that the WIC leaders, including A. I. Guchkov, cannot fairly be accused of actively planning the overthrow of Nicholas II with either the army high command or the workers, and this seems convincing.

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I. I. MINTS. *God 1918i* [The Year 1918]. (Nauchnyi Sovet po Kompleksnoi Probleme “Velikaia Oktabr'skaia Sotsialisticheskaia Revoliutsiia.”) Moscow: Nauka. 1982. Pp. 576. 3 r. 60 k.

Academician (since 1946) I. I. Mints has for decades been a leading Soviet historian. His numerous works include his three-volume *History of the Great October* (1967–73; 2d ed., 1977–79), of which Mints considers *God 1918i* a continuation (p. 5).

Mints divides 1918 into three periods. Between October 1917 and April 1918 the Bolsheviks established power, from April to June they strengthened that power, and between June and November they consolidated power (p. 395). *God 1918i* is fairly evenly divided between foreign and domestic affairs. The first and last three chapters deal with foreign affairs—Brest-Litovsk, German-Soviet and Entente-Soviet relations, and foreign armed intervention. The intervening five chapters discuss internal affairs—the elimination of counterrevolutionary parties and domestic opposition, the agrarian revolution, and the culminating Sixth Extraordinary Congress of Soviets.

Mints's theses are clear. First, Lenin was a genius. “Every step . . . was illuminated by the genius of Lenin” (p. 566), and Lenin's “Current Tasks of Soviet Power” “was . . . in its significance, a second *Communist Manifesto*” (p. 215). Second, Bolshevik actions were always correct, but those of opponents were always malevolently wrong. Thus, publication of Bolshevik peace conditions was good, but Wil-

son's Fourteen Points were "a manifesto of American imperialism, a program of struggle for world hegemony" (p. 50). Third, in every area of their activity the Bolsheviks never acted spontaneously but always in a manner planned, invariably by Lenin, far in advance (see, for instance, pp. 237–45 for industry and pp. 264–73 for agriculture). And, fourth, Mints dismisses any opposing views—Marxist or bourgeois—as "slandorous perversions [that] show the ignorance of critics who do not understand" (p. 375; see also pp. 254, 264, 276, *passim*).

This book fails to contribute to historical knowledge. True, Mints forthrightly portrays the Bolsheviks' problems. But this is insufficient. The work manifests clear problems. One is Mints's ability to see victory in defeat. For instance, Brest-Litovsk emerges as a great Bolshevik victory (p. 42), and Mints clearly implies that the Soviet economy was doing well in 1918 (p. 231). Second, nonperson history remains in force. Although there is much about the trade unions, the Supreme Economic Council, and the formation of the Red Army, there is no mention at all in the book of M. Tomskii or A. Rykov and no mention of L. Trotsky in connection with his military role. Third, Mints uses a Marxist double standard in judging events. Thus, when the Bolsheviks won majorities in soviets it expressed the popular will, but other parties' victories were the result of "all possible machinations" (p. 451). Or it was wrong for Menshevik Georgia but acceptable for Bolshevik Russia to work with Germany (p. 465). Fourth, the author frequently either contradicts himself, draws unwarranted conclusions from quotations, or fails to put an event into proper context. Thus, he argues that nationalization was fully planned out (pp. 236–38), but that the Bolsheviks had quickly to nationalize industry to avoid paying reparations to the Germans (p. 242). Or he quotes a document showing Entente knowledge of an event and concludes that this proves Entente guilt for that act (pp. 474–75; see also pp. 486–87, 532). And Mints reports that 97 percent of the delegates at the Sixth Congress of Soviets were Bolsheviks, but fails to mention that all other parties were outlawed (p. 541).

Although lacking a bibliography and a subject index, the book is well footnoted and contains a name index. As an excellent example of the triumph of *partinost'* over historical objectivity, the book should be in graduate collections.

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IU. P. BOKAREV. *Biudzhetnye obsledovaniia krest'ianskikh khoziaistv 20-kh godov kak istoricheskii istochnik* [Peasant Household Budget Studies of the 1920s as His-

torical Source Material]. Moscow: Nauka. 1981. Pp. 308. 2 r. 70 k.

Iu. P. Bokarev has produced an exceptional review and evaluation of peasant budget studies of the 1920s. It is, in fact, a tour de force and must reading for any scholar with an interest in the still-controversial questions about the viability of precollectivized Soviet agriculture.

A glance at the footnotes alone is sufficient to demonstrate the seriousness of Bokarev's effort. The statistical literature of the period is combed thoroughly, and much interesting data are presented in useful tables throughout the volume. Archival sources are prominent, and both descriptive and analytical literature of the period are covered extensively. Certain names and interpretations are missing, of course, for the issues Bokarev discusses remain politically sensitive. Overall, however, this is an important and scholarly work.

The weakest chapter, as might be expected, is the first, which treats the various interpretations of budget data that competed for the attention of policy makers in the 1920s. Some prominent polemicists, such as Nikolai Bukharin and E. A. Preobrazhenskii, are missing, but most of the serious scholars appear to be represented, including A. V. Chayanov.

According to Bokarev, the quality of peasant budget data varies for different periods. He divides the 1920s into three periods: 1919–22, 1922–26, and 1926–30. Chapter 2 explains the historical and some of the political factors that conditioned data quality and temporal comparability. Chapter 3 continues data evaluation by examining the two basic modes of data collection: the "expedition" versus peasant diaries. Data collected by expeditionary surveys have been better preserved than have data collected by means of peasant diaries. Bokarev has some interesting observations to make concerning these two types of surveys and about the quality of the data that have been preserved.

Bokarev turns in chapter 4 to a critique of various methods used in the analysis of peasant budget data. This includes the problem of categorizing and aggregating peasant households. A principal concern was, of course, measurement of economic differentiation, for which no unambiguous measure was ever established. Bokarev goes on in this chapter to consider problems of data extrapolation (to characterize regions or classes), treatment of missing observations, avoidance of autocorrelation, and even mathematical modeling of peasant-household decision making.

Having completed his methodological review, Bokarev presents his own analysis and interpretations in the remaining three chapters. Chapter 5 considers demographic processes during the 1920s and

the organization and the structure of authority within peasant households. His principal aim is to demonstrate the existence of redundant rural labor. Chapter 6 treats economic differentiation, the "zonal" character of Russian agricultural geography, and the economic viability of peasant agriculture. Chapter 7 discusses the links between peasant agriculture and the urban-industrial sector during the 1920s, and it includes as a centerpiece consideration of the "scissors crisis."

Space does not permit an evaluation of Bokarev's findings and interpretations. Suffice it to say that he has presented his results in a very neat and careful fashion, which will be highly useful to other investigators of the period, but that he has not come to a conclusion that contradicts the "standard" official interpretation of the economic and sociological conditions of the peasantry in the 1920s. Thus, collectivization is not challenged as a "necessity" for successful industrialization. Data are provided, however, that could be interpreted in support of alternatives to collectivization and to official interpretations of peasant life in the 1920s.

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D. C. B. LIEVEN. *Russia and the Origins of the First World War. (The Making of the Twentieth Century.)* New York: St. Martin's. 1983. Pp. 213. \$25.00.

D. C. B. Lieven's study is the latest in the distinguished "The Making of the Twentieth Century" series, which provides succinct accounts of important historical themes for the general or undergraduate reader. In this particular case, the text is no more than 153 pages, so an exhaustive treatment of the Russian empire, its external and internal policies, and its place in the origins of the First World War ought not to be expected. Nonetheless, what is offered here is immensely useful, and not merely for teaching purposes. Not only has Lieven had access to important archival sources in Moscow and Leningrad (and Russian-language collections in the West), but he has also delved into reports from French, British, and Austro-Hungarian representatives in Russia before 1914. The book, therefore, contains much new material and is no mere synthesis of existing literature.

More than that, *Russia and the Origins of the First World War* distinguishes itself by two welcome features. The first is the author's determination to avoid a purely diplomatic narrative and to set the story within the context of internal opinions and pressure groups, domestic changes, economic developments, and geopolitical constraints. Thus, the

chapters on "Russia as a Great Power" and "Russian Foreign Policy, 1905–1914" are followed by a brief discussion of "Who Ruled in Petersburg?" and then by "Actors and Opinions," the most substantial section of all, which looks at the emperor, his court, the elder statesmen, the Foreign Ministry and diplomatic corps, the army and navy, the major parties, the press, and public opinion. This excursus is the more important because, at the end of the day, Lieven argues persuasively that it was not domestic pressures—and especially not calculations about averting revolution—that determined Russian policy in 1914, but rather the passionate and widespread belief that Russia could not abdicate her great-power role. The sheer surge of German industrial and military power from Bismarck's time onward seemed to call into question Russia's own place in the European states system. Although this caused some Russian circles to wish to effect a compromise with Berlin or at least to deflect its expansionist drive in other directions (against Britain), the concern about Germany also helped the pan-Slavs, who were firm advocates of the French alliance and even of the British entente. Whatever the nuances, no one questioned the need to contain the Austro-German challenge from 1908 onward.

The other great advantage of this work is its style: clear, judicious, extremely intelligent sentences flow one after the other and make the book a pleasure to read. It will be for those conversant with Russian archival materials to judge just how well Lieven has handled those sources, but to this reviewer he offers a convincing and lucid account. It would have been useful to have had further details on the rearmament programs before 1914, but in a work of this length not everything is possible. With this present study, teachers of twentieth-century Europe at last have a solid but succinct account of pre-1914 Russia to press into the hands of students seeking to understand better the origins of the Great War.

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ROBERT L. HUTCHINGS. *Soviet–East European Relations: Consolidation and Conflict, 1968–1980.* Madison: University of Wisconsin Press. 1983. Pp. xvi, 314. \$27.50.

Robert L. Hutchings, deputy director of Radio Free Europe, devotes this well-documented monograph to a crucial aspect of current history: the failure of the USSR to achieve firm control over its East European satellites. Although earlier periods are summarized, this book concentrates on 1968–80, the years between Czechoslovakia's brief liberalization and the rise of free Solidarity unions in Poland.

During this era, Russia's Brezhnev regime attempted more sophisticated methods of controlling its satellites than ever before.

Earlier methods of control were primitive, because Stalin governed Eastern Europe through puppet rulers. Although he founded Comecon (CMEA), Soviet-satellites's common market, it was inactive while he lived. After his death, Eastern Europe was confused by de-Stalinization, which caused the 1956 Polish and Hungarian revolutions.

After Khrushchev crushed the Hungarian revolt, he sought better ways for supervising the satellites. CMEA became active and tried to coordinate USSR-East European economic planning. The Warsaw Pact military alliance was formed, eventually commencing joint military maneuvers. East European governments could experiment in internal affairs, as long as they supported Soviet foreign policy. Such semifreedom created the 1968 Czech liberalization, which Moscow feared and forcibly suppressed.

Then Brezhnev tried new tactics. CMEA became much more active, establishing Soviet economic links with the satellites at all levels from factories to cabinet ministries. Warsaw-Pact bureaucracy greatly expanded, and the alliance became a forum for joint diplomacy toward the West. Large-scale cultural exchange attempted to maintain Soviet-bloc ideological unity.

East-West detente was Soviet and satellite policy during the 1970s. Taking advantage of detente, Eastern Europe borrowed heavily from Western Europe to pay for imports of the latest Western machinery to modernize its industry. In turn, the modernized industries raised East European standards of living and produced export goods to pay the debts to the West.

Then, in the mid-1970s, came economic recession. Stagflation in Western Europe raised the price of Western machinery and reduced Western demand for East European goods. Eastern Europe was saddled with debts it could not repay on time. Making matters worse, the USSR raised the price for Soviet oil, the main fuel for most East European nations. Satellite economic growth slowed, and in Poland the living standard sharply declined.

Meanwhile, the Warsaw Pact had problems. Because of financial difficulties in Eastern Europe, most satellite armies decreased in size, as did the percentage of national income devoted to armament. Therefore, most East European tanks, missiles, and warplanes are old Soviet models, fast becoming obsolete. Joint maneuvers became less frequent, and only a few satellite divisions trained alongside Soviet troops.

Also in 1980, Soviet-bloc ideological unity was shattered by the Polish Solidarity movement. Thus, Brezhnev's unification measures failed in econom-

ics, armament, and ideology, leaving grave problems for his Kremlin successors.

This well-written book should enlighten both scholars and students of East European affairs.

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NEAR EAST

B. F. MUSALLAM. *Sex and Society in Islam: Birth Control before the Nineteenth Century*. (Cambridge Studies in Islamic Civilization.) New York: Cambridge University Press. 1983. Pp. ix, 176. \$29.95.

B. F. Musallam's study of contraception in the medieval Muslim world introduces the reader to a fascinating, yet peculiarly neglected aspect of traditional scholarship in premodern Islam. A field with an "embarrassment of riches" (p. vi) that has remained largely unexplored by historians, birth control and its practice may have altered the demographic profile of the Near East. At least so the author claims. The brevity of Musallam's tightly written monograph should not obscure his thorough analysis or keen insights. As an illustration of legal theory on contraception and its application, this book makes a most valuable contribution to medieval sociology and the history of science. As a persuasive case for the impact of contraception on population during the later Middle Ages, I find the work less convincing.

Musallam begins with an outline of recent trends in European historical demography, challenging its preoccupation with the relationship between industrialization and population increase as less relevant to other regions and cultures. He then proceeds with surveys of contraception under Islamic law (chap. 1), birth control and the rights of women (chap. 2), contraception theory in Muslim thought (chap. 3), Arabic medicine and contraceptive methods (chap. 4), popular attitudes toward birth control (chap. 5), and concludes with postulates on possible ties between contraception and population decline in the central states of Egypt and Syria (chap. 6).

Musallam demonstrates that orthodox Muslim jurists sanctioned contraception (withdrawal or *coitus interruptus*) on the grounds of traditions ascribed to the Prophet (*Hadith*), since the Koran was silent on this issue. Assuming that both parents contributed essential qualities to the foetus, they reasoned that any means of barring semen from the uterus was legal. Indeed, to a majority of jurists, abortion itself was also licit, since Islamic law (in contrast to Judaic law) did not regard "destruction of seed" as criminal. The combination of male and female "semen" alone produced a potential human, who became "ensouled," however, only

after 120 days of pregnancy. Musallam weighs accepted reasons for limiting births acknowledged by scholars against the positive good of procreation and the legal rights of women to children and sexual fulfillment. His interweaving of Muslim attitudes toward sexual morality (in sharp contrast with those of medieval Christendom) with the principle of a free woman's choice as ultimate regulator of contraception emerges as one of the most intriguing dimensions of the study.

Musallam's detailed discussions of medical treatises dealing with birth control techniques, pharmacological compendia (*materia medica*), and anthologies of popular writings (*erotica*) provide copious evidence of sophisticated approaches to contraception that had matured by the fourteenth century. On the matter of an embryo's origin, Musallam traces the debate between advocates of Aristotle (foetus defined exclusively by male semen) such as Ibn Sina, and supporters of Hippocrates and Galen (both male and female donations necessary) such as al-Ghazali, al-Razi, and Ibn Qayyim, noting how the latter theory won out because it upheld the *Hadith* while corroborating systematic observation of a child's inherited characteristics. The voluminous lists of contraceptive recipes, drugs, and devices available (provided in tables 1–15 and the notes but unsupported by their modern chemical equivalents) are intrinsically interesting for their variety (although ineffective in their chemical properties), but significant primarily because they focused overwhelmingly on feminine needs. However rational or fanciful they might be, these techniques recommended by physicians and pharmacologists suggest that their clientele were women for the most part. But erotica were written by men for men, and stress male strategies for avoiding a partner's conception.

Musallam's preceding survey, based on original research, amply demonstrates the importance of his study. By contrast, his closing statement on the historical consequences of birth control rests on weaker foundations. Musallam readily admits a lack of statistical evidence for accurate population estimates in the premodern Near East. He also acknowledges that his sources reflect an urban setting. He therefore concedes that his materials yield solely qualitative evidence for birth control as a factor contributing to the demographic decline of later medieval times. He builds his case around three conditions for widespread contraceptive practice: existence of rational techniques, their diffusion through Muslim society, and their public acceptance in principle. Musallam then reinterprets chronic social conditions (political turmoil, economic deterioration, plague, warfare, and militarist exploitation) in light of his data to identify plausible inducements for restricting births. A widespread desire to limit dependents, and thereby to increase a child's oppor-

tunities for prosperity, can be deduced from the perennial insecurity of the age. I agree that such motives were possible, but their impact on the birth rate, in comparison with such factors as mortality stemming from epidemics, remains indeterminate—especially if attitudes of the rural majority are untraceable. Indeed, the current indifference of modern rural populations in Egypt to family planning programs becomes even more puzzling after Musallam's statement that the Sharia permitted contraception. Nonetheless, Musallam has underscored critical questions that merit further study to clarify the phenomenon of population decline and its causes.

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CATHERINE KAMINSKY and SIMON KRUK. *Le nationalisme arabe et le nationalisme juif*. Preface by D'ANNIE KRIEGL. (Les Chemins de l'Histoire.) Paris: Presses Universitaires de France. 1983. Pp. 244. 125 fr.

Catherine Kaminsky and Simon Kruk search for the sources of and the determinative influences on the conflict between Jews and Arabs in the Middle East. They survey the formation of two nationalisms, Arab and Jewish, in the nineteenth century, which were stimulated by Europe, developed separately in Europe and the Middle East, and clashed in Palestine. They consider the policies of the European great powers, who exploited the conflict for the sake of their own interests, down to the end of World War I. The actions and interactions of British, Jews, Arabs, Soviets, and French until 1930 are analyzed.

This is primarily a work of synthesis rather than one of original research. The documentation is extensive, but it is woefully inadequately described in both notes and bibliography. There are a number of minor errors, for example, incorrect personal names, but only one major factual one—the Sykes-Picot agreement did not assign Palestine to Britain (p. 113). The authors, like a number of others, including specialists in Arab studies, are misled by antique notions into a misunderstanding of the role of Christian Arabs in an essentially Islamic Arab nationalism and to a gross exaggeration of Arab hostility to the Ottomans and the extent of Arab nationalism before 1918. Britain is described as misleading Arabs and Jews. In fact, the British were reasonably clear in promising Arabs and Zionists much less than they demanded. British error, if any, was in not giving due regard to the obvious intentions of Zionists and Arabs to act as if the British had promised them all that they claimed.

On the positive side, the achievement is great.

The authors have utilized an impressive mass of material, and they have given proper attention to all aspects of the subject. Above all, they have risen above the too common engrossment with old arguments about rights and wrongs to take a fresh look at the subject as a whole. Thanks to them, we have a better view. The 1929 disturbances, national insurrection, pogrom, and socioeconomic discontent together, the authors believe, created a decisive turn: the first schism between the Jewish community and the mandatory government; the end of attempts at Jewish-Arab understanding (David Ben-Gurion recognized that the conflict was national, not social as he previously had thought; Jews abandoned the Palestine Communist party to the Arabs); a great hardening of Arab claims; a reinforcement (but not the creation) of Palestinian Arab nationalism; and British preoccupation with the intensification of Arab nationalism and Islamic solidarity (masked by emphasis on the socioeconomic aspect of the conflict). From the beginning, the two nationalisms, Jewish and Arab, have necessarily employed the secularist terminology of contemporary European nationalism, but in fact both have been dominated by religion in the sense of "civilization." The conflict is thus one of conflicting civilizations.

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S. D. GOITEIN. *A Mediterranean Society: The Jewish Communities of the Arab World As Portrayed in the Documents of the Cairo Geniza*. Volume 4, *Daily Life*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xxvi, 487. \$38.50.

More than a century after the first documents were removed from old Cairo's synagogue, Geniza research continues to expand its horizon. No better demonstration of this phenomenon exists than S. D. Goitein's *A Mediterranean Society*. When Goitein and the late G. E. von Grunebaum initiated this project in the fall of 1958 they envisaged a one-volume survey. By the time the first volume appeared in 1967 it had been determined that three volumes would be necessary. In 1983 the fourth volume was published with the promise of yet a fifth. It seems that each door opened into the Geniza's treasures opens a passage to yet another.

Earlier volumes have portrayed the economy, the community, and the family of the Geniza Jews. In each case Goitein has mined the documents, extracted an impressive amount of information, and set it out in such a manner as to convey a sense of the reality within which these Jews lived. Again, in the fourth volume, he treats us to no less a repast.

This volume, *Daily Life*, examines the communi-

ty's material environment as a source for its social history. The first section explores aspects of the Geniza home. Goitein begins with the nature and major features of the city and its relationship with the countryside. He then turns his attention to the architectural structure of the Geniza house and describes the various types of houses, their component divisions and their uses. Of special interest here is Goitein's demonstration of the tension between individualism and conformity so characteristic of medieval Islamic civilization. After a discussion of the socioeconomic and legal aspects of housing, Goitein concludes this section with an extensive review of the contents of the Geniza home, furnishings, and housewares. The second section provides an extensive look at clothing and jewelry. Their materials, production, variety of types, and socioeconomic functions are exhaustively studied. Although it is likely that there were some distinctive aspects to the Jew's raiment and adornment, there is, nonetheless, much that can be learned about the tastes and styles of medieval Arab society in general. The third section deals with food and drink. Goitein makes it clear that the documents are not as rich in information in this area as in others. Yet, the Jews were known for their enjoyment of food and there can be little doubt that this area is basic to any study of lifestyles. Finally, Goitein provides a short but interesting section on the Geniza Jews' domestic transportation system and its sociological significance. The volume concludes with four appendixes providing lists and sample documents in translation and, covering the entire volume, extensive references and notes.

This volume is a monument, as are the other three, to Goitein's more than thirty-five-year investment in Geniza research and is a tribute to his keen mind and rigorous scholarship. As time goes on it will, no doubt, be recognized as a classic. Yet, the nature of the materials is such as to create limitations that, although not detracting from the importance or quality of the work, should be noted. Many of Goitein's interpretations are highly speculative. He himself notes this fact from time to time. There is, of course, much work yet to be done on the documentary materials and, furthermore, one cannot expect to answer all questions about a society based solely on what is, in the end, a random collection of documentary discards. These speculations, therefore, must continue to be evaluated as further research is forthcoming from both Geniza and non-Geniza sources. Goitein's work deserves no less. Finally, much of the book is highly technical, especially in the discussion and definition of terms. This material is crucial but it is unlikely to engage the attention of the nonspecialist reader. One might hope for an eventual return to the original 1958 idea after this project is completed. All in all, I am

one of many anxiously awaiting the next installment.

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JOHN J. MCTAGUE. *British Policy in Palestine, 1917–1922*. Lanham, Md.: University Press of America. 1983. Pp. x, 276. Cloth \$23.00, paper \$12.50.

The five years from 1917 to 1922 set Palestine on a fatal course of conflict between the Arab and Jewish communities. Some scholars and politicians assert that if the British government had imposed a Jewish state in the opening years of its rule the Arabs would have acquiesced, and the subsequent conflict would have been avoided. Others maintain that conflict was inevitable but that neither the British nor the Zionists recognized the depth of Arab opposition. In contrast John J. McTague argues forcefully that "Britain deliberately misled the Arabs about her post-war intentions" toward Palestine (p. 37). Moreover, he claims, "all the elements of this problem were visible by July 1922, when the mandate was approved" (p. 233) and therefore "no one can realistically claim that Arab-Zionist antagonism was a gradual development of the mandate period which could not have been foreseen" (p. 235).

McTague documents his case with a detailed chronological reconstruction of the events in Palestine as well as of the policy debates in London. He assesses the contradictory British promises during World War I and disagrees with Isaiah Friedman, who argues in *The Question of Palestine, 1914–1918*, that the Balfour Declaration was the only commitment binding on Britain. Instead, McTague maintains, the McMahon-Husayn correspondence of 1915–16 took precedence and "the Balfour Declaration could not wipe out the McMahon-Hussein letters, which definitely included Palestine in Arab territory" (p. 241).

When the British army occupied Palestine in November 1917, it tried to avoid the inherent contradictions by maintaining a military administration, subject to the rules of the Hague Convention. The British officers, however, clashed with the Zionist organization, which sought to assert a special status for the Jewish community. This demand alarmed the Arabs, who feared that they would be dispossessed if a Jewish majority and state were established. As Neil Caplan has documented in *Palestine Jewry and the Arab Question, 1917–1925*, the attitudes and actions of the Zionist activists in Palestine scared and alienated the Arabs.

By August 1919, the military administration felt that the "erroneous commitment [to the Jewish National Home] should be rectified before irreparable damage had been done" (p. 89). Nevertheless,

the foreign minister insisted that Britain maintain that commitment even though he acknowledged that it was sinking into a quagmire. Serious Arab riots in April 1920 and May 1921 and the arguments presented by an Arab delegation to London in 1921–22 underlined the inherent conflict. Not until June 1921 did an internal Colonial Office memorandum state bluntly that Britain must choose among three alternatives: governing in accord with the wishes of the indigenous population, giving the Balfour Declaration clear precedence, or abandoning Palestine (p. 164). In the end the government chose to support a modified version of Zionism, which did not satisfy Zionist aspirations, increasingly alienated the Arabs, and led to internal revolts that severely weakened Britain's strategic position. This dismal history is traced in a clear and forceful manner by McTague.

The book is marred by incredibly sloppy typesetting. No one seems to have proofread the text, which is riddled with misspellings and typographical errors.

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JACQUES KORNBERG, editor. *At the Crossroads: Essays on Ahad Ha-Am*. (SUNY Series in Modern Jewish History.) Albany: State University of New York Press. 1983. Pp. xxvii, 207.

Ahad Ha-Am was the Hebrew pseudonym employed after 1892 by Asher Ginsberg (1856–1927), one of the most prolific and influential of early Zionist theoreticians and publicists. Taken from Genesis 26:10, the phrase is usually translated "one of the people." Traditional Jewish bible commentators, however, rendered it, "one above the people." Ahad Ha-Am, who besides his several other attributes was also a Judaic scholar, was doubtless sensitive to the double-edged tenor of his sobriquet. Its self-application was definitely appropriate. Although patently—and proudly—rooted in his East European Jewish environment and education, he was hardly its typical product. Culturally, temperamentally, and philosophically Ahad Ha-Am was an elitist, not a populist. His principal means of expression were his essays—literary masterpieces of Hebrew style and astringent thought that covered a vast range of historical and topical subjects and that were written by and for a member of the enlightened Jewish intelligentsia.

Not that Ahad Ha-Am pronounced from an ivory tower. On the contrary, he was very much a political activist, who threw himself into the vortex of the affairs of the emergent Jewish national movement. A long-time supporter of Hibat Zion, he attended

the first Zionist Congress convened by Theodore Herzl in 1897, participated in the Odessa central committee thereafter, and acted as Chaim Weizmann's confidant during the Balfour Declaration negotiations. Throughout this time—and again through the medium of those brilliant, if often abrasive, essays—he attempted to influence Zionism's course. He exhorted, speculated, and (of course) criticized. Above all, he constantly reminded his colleagues that Zionism had to be a cultural movement not just a political party. Indeed, Zionists had to exercise political restraint in order to attain the essential goals of a revival of Judaism.

Conventional historical wisdom holds that Ahad Ha-Am's advocacy of a gradualist strategy was "right" (far more so than was the strategy of Herzl, Israel Zangwill, and others of his political opponents). But, notwithstanding such accreditations, the man remains an enigma. Despite all his public activity—and authority—he never became a leader in the accepted political sense. Even when at the very center of events, he seemed somehow to stand on the periphery. He shrank from the practical consequences of some of his own intellectual initiatives and refused, once again, to become "one of the people." (Significantly, the Benei Moshe—a proto-Zionist group founded on his own model in the 1890s—was a semisecret and "priestly" caste.)

The present book of articles (thirteen in all, plus a helpful introduction by the editor, Jacques Kornberg) is to be commended for addressing itself to this intriguing aspect of its subject. It is not a biography of Ahad Ha-Am; indeed, it seems deliberately to avoid discussion of his occasionally unhappy personal life. Rather, it pays detailed attention to his major interests (the Hebrew language; Jewish philosophy and history; and, of course, Zionism); his principal controversies (literary and political); and his influence (in Germany, the USA, and on Weizmann). This approach—even by its own standards—does possess disadvantages: the reader is deprived of an appreciation of Ahad Ha-Am's intellectual development; some episodes are arguably discussed too often (the Benei Moshe experiment); others too little (for example, Ahad Ha-Am's relationship with Brenner and his impact on the *yishuv*). But the volume possesses the distinct advantage of a collection of specialist studies—each of which develops its own theme, and few of which pay uncritical homage to their protagonist. All in all, it provides the general reader with evidence of the multifaceted interests of a man whose doctrines are still relevant. The specialist (who will be more likely to consult the often voluminous footnotes) will find useful indicators for further research into a character who, together with other Zionists of his generation, effectively changed the entire course of Jewish history.

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GUITY NASHAT. *The Origins of Modern Reform in Iran, 1870–80*. Urbana: University of Illinois Press. 1982. Pp. xvii, 222. \$17.50.

Guity Nashat's *The Origin of Modern Reform in Iran, 1870–80* is a study of Mirza Husain Khan Moshir ed-Dauleh Sepahsalar's innovations, which he strove to establish while holding a succession of government offices as minister of justice, war, foreign affairs, and as prime minister. In six chapters it discusses in detail the judicial, military, political, economic, and cultural reforms, using as primary sources the minister's private correspondence, government edicts, and contemporary newspapers. The book, however, does not provide a thorough study of the application and ultimate failure of these reforms. Rather than analyzing the dynamics of the process involved in implementing institutional changes, the author summarizes and briefly evaluates the reforms as ideas. Consequently the book reads as a history of ideas, though the reader is not given the necessary social context, intellectual origins, and structuralist analysis that such an approach entails.

In chapter 3 Nashat studies the juridical reforms that the Sepahsalar attempted to institute in order to curtail the arbitrary power traditionally held by provincial governors, government officials, landowners, and tax farmers, without explaining how that power was exercised. Moreover, aside from summary references to the dual *urf/sharʿ* (religious/customary or secular) legal system citing secondary Persian and Western sources, she provides no accurate description of the judicial practices and juridical theories then prevailing.

In chapter 5 Nashat enumerates the political reforms aimed at setting limits to government officials' abuse of power. She states that the mujtahids were "alarmed" without telling us exactly how the high-ranking religious leaders used their influence or how this was threatened. In chapter 6, discussing the far-reaching reforms known collectively as *Tanzimat-i Hasanah*, which in effect was an attempt at systematizing and codifying the *urf* laws, she vaguely asserts that traditionally "the ulama exercised much control over the secular courts" and thus they were quick to realize that though the code "touched only lightly" on their prerogatives, "a steadily centralizing state would eventually tolerate no authority but its own" (pp. 104–05). She gives no information as to the ulama's connection with powerful politicians whose struggle against the code the religious leaders so heartily supported. Referring to Zill us-Sultan, the Qajar prince and governor of Isfahan, as the fiercest opponent of the Sepahsalar and his reforms, she claims that there is "no documentary link between Zill-us-Sultan and the ulama," but surmises that the prince was "secretly active in inciting the ulama to work against the code," before

concluding that "In the face of this active religious opposition, the code was quietly abandoned" (p. 105).

To explain the failure of the reforms in terms of religious opposition is to miss the point. Nineteenth-century Persian sources clearly show that within the peculiar system of political alliances and alignments every major faction, be it "progressive" or "reactionary" in outlook, included individual ulama whose motives were based neither on ideological nor religious considerations. In fact the ulama never formed a single professional group or "powerful pressure group," nor did the mujtahids "avoid connection with those in power" as the author alleges (pp. 11–12). The politics of factionalism hampered the reformers but enabled the rulers to remain in power for as long as they did, despite their individual weaknesses, the incompetence of their bureaucracy, the lack of an army, and the existence of semiautonomous provincial centers of authority. How did the system work? How decisive a role did it play in the defeat of the Sepahsalar and other reformers before and after him? These are issues of central importance but are never raised in this study.

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GENE R. GARTHWAITE. *Khans and Shahs: A Documentary Analysis of the Bakhtiari in Iran*. New York: Cambridge University Press. 1983. Pp. xii, 213. \$49.50.

Ever since the eleventh century pastoral nomads have played a major role in Iranian history. Most dynasties, with the exception of the Pahlavis (1925–79), were either of nomadic origin or relied heavily on their assistance to seize power. The significance of nomads has not been limited to politics or their contribution to the military, but also has included important economic activities. They have constituted a sizable portion of the population and have been the primary producers of such staple items as wool and meat. Yet despite their significance we know little about nomadic or tribal societies: their political and social organizations, their economic activities, their relationship with some of the dynasties they helped bring to power. We also know little about the inner workings of tribal society: the way in which power is created and how it is maintained, its basic institutions, the relations between the individual and other members of the community, or its values.

Gene R. Garthwaite's *Khans and Shahs* is a timely and welcome addition to the short list of studies dealing with nomadic society. Garthwaite has combined training as a historian with fieldwork in the

area to reconstruct the political history of Bakhtiari society. This tribe, which to this day inhabits a large area in the central Zagros mountains in western Iran, was the most important nomadic confederation in the second half of the nineteenth century, and played an important role in national affairs in the first three decades of the twentieth century.

The focal point of Garthwaite's study is the rise during the nineteenth century of the Duraki Khans, the leading subdivision of one of the two major branches of the Bakhtiari, and the consolidation of power in a particular family. The process culminated in the suzerainty of Husain Quli Khan, who ruled the newly united confederation without challenge from 1862 to his death at the government's order in 1882. His ascendancy was recognized by the government in 1867 when it conferred on him the title of *ilkhani* (paramount tribal leader). After his death no other leader maintained as much power for so long, even though his descendants retained the tribal leadership.

Garthwaite also recounts the reasons for and nature of the involvement of Bakhtiari leaders in national affairs, particularly in the Constitutional Revolution and the ensuing civil war (1907–11). Finally he discusses the impact of the centralizing policies of the Pahlavi dynasty and its effort to create a modern nation state on the Bakhtiari leadership and on the tribe.

Garthwaite has drawn on an impressive array of sources in this work. The most important is a collection of hitherto unpublished documents, including a diary of Husain Quli Khan Ilkhani, to which he was given access by members of the Bakhtiari family. The recounting of political events, however, is only one part of what this study offers. Garthwaite uses the narrative to shed light on many important issues. The first two chapters present a detailed discussion of the nature and problems of tribal leadership and the social and economic structure of the tribal organization. The study also sheds light on the complex relations between the tribe and the central government. Discussion of the early stages of British involvement in internal Iranian affairs, following the discovery of oil in the Bakhtiari region at the beginning of the twentieth century, also makes fascinating reading. This valuable study will be of use not only to specialists in Iranian history but also to the general reader with interest in the pastoral societies of the Middle East.

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FUAD I. KHURI. *Tribe and State in Bahrain: The Transformation of Social and Political Authority in an Arab State*. (Publications of the Center for Middle Eastern

Studies, number 14.) Chicago: University of Chicago Press. 1980. Pp. 289. \$16.00.

Fuad I. Khuri's book is the most detailed analysis of political authority that exists today on Bahrain, a tiny nation state of two-hundred and forty-thousand persons in the Persian Gulf. Indeed, it ranks as one of the few good studies of any of the small Persian Gulf sheikhdoms that only began to assume their present autonomous political structures in the eighteenth and nineteenth centuries. This ensures that *Tribe and State in Bahrain* will remain a basic reference not only for historians, but also for anthropologists, political scientists, and even those in international relations.

Khuri's study focuses on two themes. One is the irreversible impact on political authority of colonial rule, a presence that directly imposed itself until 1970, and of rapid socioeconomic change. In the first instance a government bureaucracy was created by a foreign power to plan development programs that increasingly articulate interest groups sought to modify. In the second instance, an economy reliant on pearl diving and date palm cultivation was quickly transformed into one based on oil.

Another theme is the impact of these same processes of colonialism and socioeconomic development on the relationships between Bahrain's tribal, peasant, and urban societies. As an anthropologist, Khuri takes considerable effort to demonstrate that these societies—perhaps better termed communities—are not separate evolutionary "ecosystems," but exist simultaneously within the same economic and political structure. The additional distinction between adherents of Shi'i and Sunni Islam remains a pervasive influence not only on these communities, but also on class, tribal, familial, employment, residential, club, and other social affiliations in Bahrain.

Two of the most interesting chapters are on parapolitical institutions, such as funeral houses (Shi'a), clubs (Shi'a and Sunni), and on organized protests and rebellions. Because Bahrain's ruling family, the Al Khalifa, forbids political organizations such as labor unions or political parties, individuals and groups are forced to be increasingly innovative if they are to influence government policies. Khuri illustrates the fact that groups at times can effectively manipulate political change within the structures of existing social affiliations. This is especially true of Shi'is and Sunnis who, holding different concepts of political legitimacy and obedience to established government, remain clearly distinguished within social institutions.

Despite meticulous attention to the details of Bahrain's social organization, the book has several flaws that seriously limit its use. One is Khuri's repeated reference to social science theorists to

justify his categories and terminology. This is very distracting to the reader who can easily discern the book's substance. Another is inadequate maps; many place-names in the text cannot be identified at all. A final and unforgivable sin is the index. In a book whose chapters are arranged topically rather than chronologically and that contains a plethora of Arabic proper names and words, a detailed index is absolutely essential. When readers attempt to alleviate their confusion by cross referencing, the index more often than not omits the reference. Frustration multiplies when references, such as the cadastral survey, are mentioned, but their relevance not explained until their third or fourth appearance. A careful editor would have been of inestimable value. These points present an almost insurmountable obstacle in broadening the appeal of what is otherwise a stimulating and rewarding book.

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AFRICA

MICHAEL MCCARTHY. *Dark Continent: Africa As Seen by Americans*. (Contributions in Afro-American and African Studies, number 75.) Westport, Conn.: Greenwood. 1983. Pp. xxviii, 192. \$27.95.

Michael McCarthy's volume in the "Contributions in Afro-American and African Studies" series deserves an important place in the growing body of scholarly literature devoted to what the series advisers term "the image of the black in Western consciousness" (p. xii). Although this excellent book touches on diverse aspects of African-American relations, it is primarily concerned with the origins and development of American perceptions of an alien environment and its people—perceptions, as McCarthy amply demonstrates, that were shaped as much by expectations and predispositions of Americans traveling and living in Africa over almost a century and a half as by the actual characteristics of the land and people they encountered there. Through a careful examination and sophisticated interpretation of the voluminous writings of dozens of Americans, McCarthy explains how a "language of discourse" about the continent and its people evolved in the United States and ultimately shaped the nation's ideas about both in a particular way.

To emphasize the complex, multidimensional nature of American images of Africa, the work is organized thematically around recorded observations and evaluations of the natural, built, symbolic, and cultural environments of the "dark continent." The highly unfavorable image of Africa, collectively projected by well-known commentators including

Henry M. Stanley and Paul Belloni Du Chaillu as well as by obscure missionaries, tended to lend support to ideas of black inferiority held by white Americans. The view of Africa that became dominant, and which was often shared by Afro-Americans, described it as a land of wild, exotic landscapes and fever-producing climates, intellectual backwardness, and economic retardation; a land whose native inhabitants were an "ignoble," morally depraved people devoid of modesty and close to cannibalism. Most American observers, according to McCarthy, bifurcated a continent of many elements into two distinct landscapes: the static, backward Africa of indigenous people; and the European Africa of progress, growth, and expansion. Despite the efforts of George Washington Williams and a few other black Americans to reconstruct the African past and call attention to positive attributes of the continent, the practice of viewing Africa with a double vision that emphasized the "savage" and unprogressive nature of the native population found widespread acceptance in geography textbooks, children's literature, and even in certain segments of the black American press. Arguing that perceptions influence behavior, McCarthy concludes that the distortions and misconceptions about Africa and Africans not only made it easier for white lynchers to slip the noose around black necks in the United States but they also reinforced the negative self-image of Afro-Americans.

Although a more comprehensive coverage of Afro-American perceptions of Africa and Africans as revealed in the columns of Bishop Henry M. Turner's *Voice of Missions*, the African Methodist Episcopal Church's *Christian Recorder* and other black American publications would be desirable, this work nonetheless provides rich insight into the complex relation between the physical and metaphysical "Africa." The significance of McCarthy's study is much larger than its brevity suggests.

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CLAIRE C. ROBERTSON and MARTIN A. KLEIN, editors.
Women and Slavery in Africa. Madison: University of Wisconsin Press. 1983. Pp. x, 380. \$22.50.

The publication of this series of essays raises a serious question concerning the validity of rushing into print with topical issues: in this case, women and slavery. In *Women and Slavery in Africa*, Martin L. Klein, noted for his work on slavery in the northern Sudan, has teamed up with Claire C. Robertson, whose only major research interest has been Ga market women in Accra. In fact, her dissertation mentions only twice the one slave woman whom she said she interviewed in her large

sample of market women (pp. 144, 155). The experience with this slave woman seems to have led her to conceive the volume under review and to her weak, uneven case study of a single slave woman in this collection. Moreover, whereas Suzanne Meirs and Igor Kopytoff produced a thoughtful, theoretical, and lengthy introduction to their *Slavery in Africa* (1977), Robertson and Klein have thrown together a mere sixteen and one-half page introduction that they claim was based on the 144 references that they cite. Yet these citations do not include the essays in the volume that get cross-referenced among the few references to their extensive bibliography. What results is a mass of confusion and very little insight into the overall topic of women and slavery in Africa.

Martin Klein's role seems to have been to gather and translate two articles from France. One of these has little bearing on women slaves; the other is by French Marxist, Claude Meillassoux, who has written widely on women and slavery in the western Sudan. Unfortunately, his article is mostly a rehash of earlier arguments on whether women slaves served in productive or reproductive roles in African societies. The editors assume that Meillassoux has read all of the essays in this volume (as some other contributors apparently have—but not all, for some reason), and in this context, Meillassoux interweaves some references to these pieces in what might be described as a "second introduction," but from a Marxist perspective. Klein, not having worked on women and slavery in the past, falls back on Meillassoux in a "me too" piece which follows.

Herbert Klein, writing on "African Women in the Atlantic Slave Trade," merely repeats general demographic data that both he and Barry Higman have published elsewhere (and which Klein cites in this volume). He tells us nothing new. Margaret Strobel's contribution, in slightly revised form, was originally accepted by the *Kenya Historical Review* in 1978 (which she notes in her article), but, because of publishing difficulties, that piece became available only a few months before it appeared in print here.

The most remarkable selection is the document discovered by Edward Alpers and beautifully interpreted in this volume. The young Yao girl, Swema, was sold into slavery in East Africa, and after arrival in Zanzibar, told her pathetic story to Catholic missionaries. From Swema we learn a good deal about the slave trade from Central Africa to the coast. Unhappily, for those who do not read French, the document itself, which follows Alper's interpretation, is in French.

Of special merit, too, are the unrelated (except by the broad topic) pieces by Robert Harms ("Sustaining the System: Trading Towns along the Middle Zaire"), Marcia Wright, with her third original contribution on women and slavery in Central Africa,

and the work of George Brooks on West African women. Brooks, indeed, might be called the Carl Degler of African women as a result of his pioneering work on *signares* in Senegal (*nharas* in Guinea-Bissau here). Also worth noting, although out of place in a book filled with nonmatches, are the articles by Edna Bay and Carol MacCormack.

The editors seemed in such a hurry to rush into print that they did not take time to compile a list of who their contributors are, or where they are. By casting a wider net, or, better yet, by commissioning pieces within a single theme under the rubric of women and slavery in Africa, the editors might have made a contribution to an area that is just beginning to receive much-needed attention.

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MICHAEL M. LASKIER. *The Alliance Israélite Universelle and the Jewish Communities of Morocco: 1862–1962*. (SUNY Series in Modern Jewish History; Publications of the Diaspora Research Institute, Tel-Aviv University, number 45.) Albany: State University of New York Press. 1983. Pp. xvi, 372.

Michael M. Laskier, of Tel-Aviv University, is the first to deal comprehensively in English with the activities of the *Alliance Israélite Universelle* in Morocco between 1862 and 1962. He uses mainly the records of the Jewish organizations involved there, secondary works, and interviews with officials and alumni of the *AIU* schools.

The Jewish communities of Morocco derived mainly from the Diaspora, Berber conversion, and the Christian Reconquest of Spain. Though numerically the largest in the Muslim world (100,000 in 1900 and 210–240,000 in 1956), they were neither as affluent nor as socially cohesive as those in Egypt, Syria, Iraq, and Algeria. The *AIU* was founded in Paris in 1860 by businessmen and independent professionals to work for the emancipation and moral progress of the Jews, particularly by disseminating modern civilization through the instrument of the French language; to lend effective support to Jews suffering injustice and discrimination because of their faith; and to awaken Europe to the Jewish problem. Whereas activities in support of the latter goals occupied much time and energy in the preprotectorate period—during which the sultan's government could not prevent local officials and rural tribesmen from abusing Jews—educational and charitable activities were primary in all periods.

The *AIU* helped to create a westernized urban elite through a program in its schools that included a modern education in French as well as a traditional Jewish one under local rabbis. In the Spanish and international zones after 1912, Spanish was also

taught. The *AIU* became the single most important agency for the education of Jewish youth. By 1939, over half of them were enrolled in its institutions, while 60 percent of adult Jews were literate compared with only a small percentage of the Muslims. Jews were found in commerce, administration, the professions, and modern manual vocations. There was even a class of prosperous Jewish farmers. The *AIU* aided the emancipation of Jewish women both through education and its efforts to end marriage of pre-teenagers. Until 1930 Jews alone had the education, including linguistic skills, necessary to serve colonial needs. After that, the increase in the numbers of European settlers and the promotion of Muslims decreased their relative importance. On the eve of Moroccan independence in 1956, the *AIU* had 33,100 pupils in 143 schools.

After 1903 the *AIU*, which sought to improve the condition and status of the Jews within their own countries, faced competition from Zionists (including organizations from the U.S.A. after 1942), who sought to promote emigration to Palestine. The end of European rule—together with the worsened Muslim-Jewish relations resulting from the circumstances of the founding of Israel—increased the insecurity of the Moroccan Jews to the point that three-fourths of them emigrated to France, Israel, and the Americas. The rest remained and the *AIU* continued to operate schools under the Arabized name *Ittihad*, for the first time offering Arabic as part of the curriculum.

Laskier has done an effective job of relating the activities of the *AIU* to the modernization process in the Jewish communities, and to the broader issues of government during the periods of the protectorate and immediate postindependence. His comments on Jewish-Muslim relations in the colonial situation are particularly insightful.

DAVID E. GARDINIER
Marquette University

ELIAS N. SAAD. *Social History of Timbuktu: The Role of Muslim Scholars and Notables, 1400–1900*. (Cambridge Studies in Islamic Civilization.) New York: Cambridge University Press. 1983. Pp. viii, 324. \$49.50.

This study updates the old, romantic image of Timbuktu—the splendidly isolated medieval university city on the southern fringe of the Sahara—into a more credible picture of a thriving commercial town which, governed by a self-perpetuating yet ethnically diverse community of scholars, maintained its autonomy within the changing state structures of the Western Sudan.

The author, arguing that “in every society tradi-

tions tend to have their own history," traces five hundred years of change within the tradition of Timbuktu's scholar-elite. His argument is organized into a neatly symmetrical format: bound between substantial introductory and concluding chapters, Saad's second and sixth chapter provide the narrative history of the city and its scholar-elite, while the middle three chapters provide cross-sectional analyses of Timbuktu's community of scholars as a learned elite, as administrators, and as regional notables.

In one sense, this is social history in the old tradition. The sources restrict the scope of the author's inquiry, and preclude us from learning much about everyday life, family, demography, and work. In another sense, however, Saad has combined the advantages of three fields of study: the Orientalist tradition, focusing on retrieval and analysis of Arabic documents; the Africanist tradition, with its multidisciplinary approach and an analysis of ethnic diversity; and the tradition of social history, from which he derives an emphasis on explicit presentation of assumptions and, to a lesser degree, research methods.

Documentation is strongest for the height of Timbuktu's power in the sixteenth century—the era of Songhai hegemony just preceding the Moroccan invasion of the Sudan. The estimated total of 150 Qur'anic schools within the limits of the sixteenth-century city suggest a total population of fifty thousand inhabitants. Saad has drawn on documents in private collections and in museums in West Africa, North Africa, and Europe, in addition to the well-known chronicles, *Tārīkh al-Sudān* and *Tārīkh al-Fattāsh*. He is graceful in his citation of earlier work on the city, but goes beyond synthesis of these studies to provide a new and elaborated interpretation. In an imaginative reliance on the genealogical emphasis in Muslim documents, he has constructed an analysis that emphasizes the multiethnic basis of Timbuktu society. Thus, in the era of the city's expansion, he distinguishes periods of dominance by Malinke, Maghsharen, and Songhai and, in the era of decline, periods of dominance by Ruma, Tadmekkat, and Lobbo: three of these are Sudanic peoples, two are Berber, and the Ruma were of Moroccan origin. Similarly, he is able to suggest distinctions in occupation and status within the scholar-elite.

The book is written with remarkable clarity. Although it is not a work of great analytical depth, it is nonetheless a careful work of synthesis, in which data were lovingly assembled into a clearly drawn portrait of the rise and decline of this city, whose importance in the history of West African Islam it reaffirms convincingly.

PATRICK MANNING
Northeastern University

D. J. E. MAIER. *Priests and Power: The Case of the Dente Shrine in Nineteenth-Century Ghana*. Bloomington: Indiana University Press. 1983. Pp. xiii, 258. \$22.50.

Priests and Power is a study in the local history of Kete Krachi, Ghana, that touches upon and elucidates numerous themes of broad interest and import. The book focuses on what D. J. E. Maier calls the "translation" of religious power into political power, particularly the political ascendancy and demise of the Dente Bosomfo, chief priest of the Dente oracle. The influence of this priest lay not only in the awesome power attributed to the oracle, acknowledged even by the rulers of the Asante empire who consistently consulted it, but also in the economic advantages of its location amidst fertile agricultural land and astride an important North-South trade route. The Dente shrine, therefore, "had great potential for achieving ideological control over people and hence economic and political control over the land, labor and important trade routes through Krachi" (p. 60).

This potential was most fully achieved in the last quarter of the nineteenth century, during the incumbency of Kwasi Gyantrubi as Dente Bosomfo, when the oracle played a central role in the organization and direction of the Bron confederation—a defensive alliance formed in reaction to Asante imperial power. Gyantrubi fully exploited his position of political influence—the Dente oath had been sworn to bind member states to the new confederation, and the oracle priest thus assumed the duties of policy enforcement, even to the extent of capital punishment for extreme dissenters. The shrine, always a sanctuary for persons seeking protection, now became a safe haven for refugees from Asante as well as a base for political conspiracy against the empire. With the decline of Asante power, trade through Krachi increased. Gyantrubi was able to supplement his income from consultations and litigations performed in the name of Dente with increased tolls and taxes on the trade. He purchased firearms and built up a personal army. The 1870s saw the arrival of Muslim merchants from the North who established their own market and settlement (*zongo*) in Kete; this community grew apace in subsequent years placing new demands on the local economy, especially in the area of food production. Gyantrubi led the response to this development, offering farmland to immigrants and putting many slaves, who were tied to the Dente shrine, to work on the land.

By the 1890s, Kete-Krachi had developed into something of a boomtown, with all the tensions that such rapid change implies. Gyantrubi feared the growing power of the Muslim community; the Muslims resented his exploitative methods of taxation and political control, which sometimes extended to

violence and even murder. The Muslims appealed first to the British for protection, and later to the Germans. The British sought to impose peaceful relationships among the factions by agreement; the Germans were more direct. Having decided to occupy Kete-Krachi, and after a violent incident of looting by Krachis in the Kete Market, they captured Gyantrubi and publicly executed him by firing squad for the crimes of theft and murder.

The complex relationships among these and many other political factions in Kete-Krachi are lucidly described by Maier in intricate detail. And her interpretations of events are often intriguing; for example, she views the Muslim merchants and Gyantrubi as, in effect, pursuing contrasting policies of development: the one entrepreneurial and based on the accumulation of private capital, the other based on the diversion of private income into state management of agriculture and support for an aggressive foreign policy. The final chapter traces the persistence of local factional strife through the twentieth century, which brings the book to a conclusion of particular contemporary relevance.

LOUIS BRENNER

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JANE L. PARPART. *Labor and Capital on the African Copperbelt*. (Class and Culture.) Philadelphia: Temple University Press. 1983. Pp. xv, 233. \$29.95.

With some reference to developments before and after, this work focuses on the period 1926–64, that is from the start up of copper production in Northern Rhodesia to the withdrawal of the colonial state. It is superbly grounded in company, state, and mission archives but is also derived from an extensive set of interviews with former labor leaders, corporate officials, and government bureaucrats. A nuanced Marxist contribution to African labor history, it examines “the interrelationship between management strategies, the work process, living conditions, and worker responses,” and asserts that “the attitudes and behavior of the copper miners are best understood as a form of class consciousness and class action” (p. 4).

By not invoking structural determinism in a heavy-handed way, Jane L. Parpart is able to discover ambivalent and varied worker responses to the demands and rewards of the work place. Thus, the reasons for and the implications of the emergence of a predominantly stabilized work force, in contrast to migrant labor employed elsewhere in Southern Africa’s extractive industries, are nicely probed. Labor factions are also emphasized. For example, the instrumental role of clerks in the first serious

black labor protest of 1935, of skilled elites in the black worker strike of 1946, and of white unions as role models for black organizations, are stressed.

Since an extensive literature on Zambian (formerly Northern Rhodesia) copperbelt labor already exists, the author chooses not to provide much detail on compound life, working conditions, or strike activity which, given the depth of her own research, seems a pity. She has been at pains, however, to point out where her findings differ from previous interpretations. For example, she challenges the suggestions that “the privileged position of the more skilled sections of the black work force led to a narrow trade union consciousness, and little political consciousness” (p. 160). She finds that the miners oscillated between involvement and noninvolvement in politics and between disunity and solidarity and that these responses were largely dictated by coercion, actual or threatened, of capital and/or the state. Indeed, although the congruence of interest between business and government is stressed, the author does allow for differences in their respective agendas and even between the two corporations (Rhodesian Selection Trust and Rhodesian Anglo-American Limited), which controlled the mines. Yet corporate consciousness and action are not always treated satisfactorily. Corporate paternalism is seen as a cynically employed strategy to maximize profits while the willingness of capital to resort to the stick for the same purpose is also stressed. This vacillation is not as plausibly explained as labor’s ambivalent responses and material desires.

Nonetheless, this exemplary study points out that sophisticated Marxism and diligent research can make a very important contribution to our understanding of African labor history.

ROBERT KUBICEK

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ASIA AND THE EAST

K. C. CHANG. *Art, Myth, and Ritual: The Path to Political Authority in Ancient China*. Cambridge: Harvard University Press. 1983. Pp. x, 142. \$18.50.

The central thesis in K. C. Chang’s analysis of the rise of the Chinese state is that it was inseparably linked to the appearance of urbanism and civilization. Consequently, towns and cities and the accumulation and concentration of wealth were closely tied to the evolution of political power. This may sound self-evident. Various scholars, however, have posited other, more exotic factors as crucial to the rise of the Chinese state. The value of these short essays lies in their cross-disciplinary perspective, in

the freshness of some of the ideas, and in the juxtaposition of the elements of the analysis.

Chang posits seven elements in the rise of political authority. These include a hierarchy of kinship units (clans and lineages); a network of regional polities (cities and towns), hierarchically integrated—at least initially—along the lines of the kinship units; coercive power (bronze weapons and chariots); meritorious deeds, real or mythological; writing as a tool for recording kinship ties, for communication with the ancestors (access to their wisdom) and for predictive purposes; exclusive access to the spirits through shamanistic practices, with animals as intermediaries; and wealth.

In the course of this presentation, Chang takes the opportunity to summarize a number of recent reinterpretations on key questions in early Chinese history and culture. Surely the aspect of Chang's analysis that will elicit the most attention is his discussion of the nature and role of shamanism in early Chinese culture, especially as it relates to political power. Textual references to shamanism in the Yangtze region have long been noted, raising the question of their relative absence in relation to the northern culture of the Yellow River basin. The author presents a convincing case for a highly developed shamanistic tradition in the North. His reinterpretation of zoomorphic artistic motifs, particularly those on the bronze sacrificial vessels, as connoting an important intermediary role for animals in shamanistic communication with the spirits should be studied by all students of the period.

Readers would also do well to pay attention to several aspects of this discussion that, by virtue of the interdisciplinary approach employed here, result in new perspectives on old issues: especially the roles of myth, writing, and marriage patterns in early Chinese political culture.

Specialists in various topics may well find issues to argue with here, but this is to be expected. There is, however, one methodological issue that causes this reviewer some uneasiness. Chang routinely uses Chou texts as evidence for Hsia and Shang circumstances and outlooks. Although archaeology has substantiated many elements of these texts in recent decades, somewhat greater caution might still be exercised. The extrapolation to Hsia and Shang times of the Chou-period emphasis on moral authority in government is a case in point. Although some element of moral authority may have been present in earlier times, we have no evidence for this except the Chou texts. It is entirely possible that the other elements enumerated by Chang as the bases for political authority may have sufficed in pre-Chou times. In other words, the emphasis on morality could well have been a Chou innovation.

A final note: nonspecialists may wish to use library copies of this volume. Seventy-five pages of

text, at this price, will probably make it attractive only for specialists.

BARRY B. BLAKELEY
Seton Hall University

BURTON PASTERNAK. *Guests in the Dragon: Social Demography of a Chinese District, 1895–1946*. New York: Columbia University Press. 1983. Pp. x, 190. \$35.00.

Studies of Chinese family organization have undergone fundamental changes within the past half century. The earliest studies were conducted within a Sinological mold and used classical Chinese texts to describe family ideals as if they were social reality. So, for instance, these writings gave the impression that most Chinese lived in large, extended families. After the 1930s anthropologists began to conduct ethnographic studies in actual communities; these studies dispelled some of the myths, showing that in fact most Chinese families were relatively small and nuclear. But they still tended to end up with discussions of the Chinese family as if it were fairly uniform in time and space.

Then in the 1960s Arthur Wolf pioneered a new line of research that transformed our understanding of Chinese family life even more fundamentally. Using Japanese colonial-era household registration data from one local area in Taiwan, Wolf showed that two marriage forms that conventional wisdom said were despised by the Chinese—minor marriage, in which a young girl is adopted and raised by a family as a future bride for their son, and uxori-local marriage, in which a groom marries into his bride's family—were not merely quite common but perhaps even preferred in that area to the supposedly ideal "major" form of marriage, in which an adult bride moves into her husband's family. In his ingenious mining of these household registration data, Wolf discovered many other anomalies, and his research made it quite clear that it no longer made sense to talk about the Chinese family, since family forms could vary quite dramatically from one time and place to another. Yet his work left us largely in the dark about the causes of such variation.

In this context, Burton Pasternak's book is a welcome addition. In his previous work he has been centrally concerned with explaining variations in Chinese family forms, and in this new study he adopts Wolf's methods but uses them to carry out a systematic comparison of three different Taiwanese villages in the colonial period. He includes Wolf's village, where minor marriages were particularly common, as well as one village where uxori-local marriages were unusually common and another where the major form of marriage was overwhelm-

ingly predominant. The contrasts across these three communities enable Pasternak to unravel mysteries not only in regard to marriage customs but also concerning divorce, fertility, adoption, and mortality and how and why these varied over the Chinese landscape.

This slim volume is packed with tables and statistics, making it somewhat tedious reading to those who prefer ethnographic vignettes to numbers. But those who make the effort will be amply rewarded, for Pasternak's accomplishments in explaining Chinese family variations are considerable. He argues, for instance, that it mattered less what sort of Chinese lived in a locality (Hakka or Hokkien) than it did what the historical patterns of intercommunity violence and irrigation and cropping patterns in the area were. Given the scale of Chinese society, a comparison of three villages in one small corner of that society may seem like a modest effort. But Pasternak's research is still a major step forward toward the eventual goal of achieving a full understanding of how and why family organization varies over the face of China.

Impressive as this tightly argued volume is, it can perhaps be faulted for remaining too "Sinocentric." Pasternak is still concerned with puzzling out the mysteries of Chinese families, and he pays no attention to the issues, debates, and methodological disputes that have arisen in the burgeoning research on historical demography of other societies, particularly in Europe. Historical demographers concerned with other parts of the world will still find this volume of interest, but they will have to make the mental bridges to their own work and concerns by themselves.

MARTIN KING WHYTE
University of Michigan

LEE FEIGON. *Chen Duxiu: Founder of the Chinese Communist Party*. Princeton: Princeton University Press. 1983. Pp. xv, 279. \$25.00.

Chen Duxiu, the foremost intellectual leader of the New Culture Movement, was cofounder of the Chinese Communist party, leader of the party for the first years of its existence, and centrally involved in other major political and intellectual movements in early twentieth-century China. Yet this work is only the second book-length study of Chen to appear in English.

The first was Thomas C. Kuo's *Ch'en Tu-hsiu, 1879-1942, and the Chinese Communist Movement* (1975). Kuo's book emphasized events and documents, and it focused on Chen as a case study of a leftist intellectual in the context of the Chinese communist movement. Lee Feigon, by contrast, is not concerned to trace the personalities and ramifi-

cations of specific political events but, rather, to trace the evolution of Chen's thinking, with particular attention to continuities and discontinuities.

But Feigon also corrects and amplifies data found in earlier writing about Chen, including Kuo's book. He shows that Chen was not the lonely orphan he claimed to have been and that he did not go to France in 1907-09 but spent those years—hitherto obscure to biographers—in Japan. Feigon describes Chen's terrorist activities, a phase neglected by Kuo. Chen began to develop ideas similar to those of Trotsky before his expulsion from the Communist party, not in reaction to that expulsion. With regard to these and other points, Feigon's work is a useful corrective.

But Feigon's most important contribution is his analysis of Chen's intellectual life, and, in general, he emphasizes the lines of continuity that can be discerned in Chen's thought and career. Feigon argues that Chen relied on tradition even while attacking it. For example, he used teacher-student relations—traditionally a strong Confucian bond—to organize political movements. He linked the movement to write in the vernacular—a movement he launched a decade before the well-known Literary Revolution—with traditional Chinese vernacular literature. Chen's moral passion was partly fired by the flame of Confucian moralism. Moreover, Chen's iconoclasm grew naturally out of his acceptance of the ideas of Chinese iconoclasts like Gu Yanwu, who had developed their ideas before Western influence was perceptible in China; thus, even his iconoclasm was partly rooted in Chinese sources. And Chen used his knowledge of the Chinese tradition as a weapon to discredit the views of less radical iconoclasts, such as Kang Youwei.

Feigon rejects the view of Chen as an uncritical Westernizer. For example, Chen saw socialism as an alternative to capitalism that was consonant with elements in the existing Chinese social structure. He was concerned not only to adopt elements of Western civilization but also to avoid the ills of Western society.

Feigon's arguments have more dimensions and subtlety than the above summary description suggests. The work is slightly marred by Feigon's insistence on pointing out his predecessors' shortcomings. Nonetheless, it is an excellent book that reveals more about Chen than any other study in English and connects his activities in interesting and revealing ways with enduring themes in Chinese history.

JAMES E. SHERIDAN
Northwestern University

RONALD P. TOBY. *State and Diplomacy in Early Modern Japan: Asia in the Development of the Tokugawa Bakufu*.

(Studies of the East Asian Institute, Columbia University.) Princeton: Princeton University Press. 1984. Pp. xxviii, 309. \$30.00.

For students of Tokugawa Japan and East Asian state relations, this book is important as an independent accomplishment and as a major element in a newly revitalized area of scholarship.

As an independent accomplishment, Ronald P. Toby's book rectifies a severe imbalance in our understanding of Tokugawa foreign relations. Whereas the extant English-language corpus focuses on relations with Europeans, Toby casts a sharp beam on the heart of Tokugawa foreign affairs: relations with Korea, China, and the Kingdom of the Ryukyus. Moreover, the study links foreign policy to domestic politics with unexampled sophistication. Toby argues forcefully that the central function of this policy was to enhance the regime's domestic political prestige, thereby strengthening and perpetuating its "legitimacy."

The core of Toby's study is four meticulously researched and skillfully organized chapters that examine the main developments in Edo's Asian policy from Ieyasu's day to the eighteenth century. He discusses the Tokugawa restoration of diplomatic links to Korea and the Ryukyus, the subsequent routinization of relations with Korea, Tokugawa policy toward the Manchu conquest of China, and the avenues of contact with the mainland. His study makes clear the unsettled nature of seventeenth-century policy and the enduring importance of foreign affairs to the shogunate, giving us an interpretation quite at odds with the older notion that Japan was "isolated" from the world after the 1630s. And it reveals the many subtle ways (choices of titles, use of era names, mode and phrasing of correspondence, deployment and nondeployment of missions, types of gifts, and so on) in which shogunal leaders exploited the rituals of diplomacy and the principles of Confucian interstate relations to advance their pretensions to authority.

For reasons of length, Toby does not examine Japanese trade with the mainland. But his brief references to it suggest that the trade was sustained, substantial, and deserving of independent book-length study.

Toby's opus is also important as one element in a reviving field of study: seventeenth-century Japanese history. In the 1950s and 1960s, scholars published a number of studies of sixteenth- and seventeenth-century institutional history, the best of them reissued in book form by John W. Hall and Marius Jansen. During the following two decades the subject languished. Recently, however, several new works on the sixteenth century have appeared. These, most especially Elizabeth Berry's lively study of Hideyoshi, argue that the key arrangements of

the Tokugawa political system were forged in the late sixteenth century. Toby's research, together with forthcoming studies by Herman Ooms and Philip Brown, strongly suggest that, although late sixteenth-century developments determined the *initial* shape of the Tokugawa settlement, it was ideological and organizational changes of the seventeenth century that converted the battlefield truce of 1600 into that enduring order called the Great Peace.

CONRAD TOTMAN
Yale University

RICHARD RUBINGER. *Private Academies of Tokugawa Japan*. Princeton: Princeton University Press. 1982. Pp. xvi, 282. \$26.50.

In a time when Japan is increasingly an object of study as a model for advanced Western industrial societies, it is somewhat unusual to encounter a book that harks back to concerns of the 1950s and 1960s: "development" and "modernization." Nevertheless, illuminating the "preconditions that may have contributed significantly to Japan's modern development" is the basic goal of Richard Rubinger in this study of Tokugawa-era private academies. It is the author's thesis that these academies "alone of Tokugawa educational institutions, contributed significantly to breaking down regional barriers and to the development of a more unified, integrated, and 'national' culture" (p. 15).

The author's definition of private academies is essentially negative. It excludes the equally private *terakoya* (common schools) and those institutions with clear sponsorship and control by the Tokugawa or individual fiefs. What remains is a category including one-person calligraphy and martial arts schools run as family businesses, groups of young political activists studying under a charismatic personality such as Yoshida Shōin, and some fairly large-scale operations that catered to those seeking the structured acquisition of culture, usually Chinese.

Overall, there is little to link these schools other than the fact that they were lumped together in the early Meiji era by the compilers of Rubinger's most important source, the government-sponsored *Nihon kyōiku-shi shiryō* (Materials on the History of Japanese Education). The author does little more than select several subsets from this source, arranging these into three case studies concerned with academies specializing in Chinese, Dutch, and other studies.

The treatment of these schools is essentially administrative. Admissions, grading, financing, and discipline get the most attention. Intellectual content is discussed only in administrative terms: texts used and hours devoted to subjects. This information

does not notably increase our understanding of the changes taking place in Japanese thought and behavior, and it did not convince me that the private academies were agents of change, although they may have been symptomatic of change.

This would have been a much stronger book if the author had given more attention to the content of education and if he had ventured beyond his Japanese collected sources. For example, Shimazaki Tôson's famous autobiographical novel *Yoakemae* (Before the Dawn) has some very vivid descriptions of the thought and behavior of nativist (*kokugaku*) scholars on the eve of the Meiji Restoration. Rubinger should also have given more attention to the official schools. A number of noted figures in the early Meiji enlightenment came out of very orthodox backgrounds, including the Tokugawa's own Shôheikô.

Overall, the book is well written and free of jargon. Two stylistic quirks do, however, deserve comment. I found the author's use of the "imperial we" somewhat distracting, especially as a part of a prelude ("we shall also argue . . ."). More important, some of Rubinger's tables present percents calculated to one or two decimal places. This gives an aura of precision not justified by the underlying numbers.

This book is handsomely produced with numerous photographs of schools and teachers, maps showing the geographical distribution of students, and ideographs for Japanese names and terms. One can only wish that this elaborate production was backed by more substance.

EARL H. KINMONTH
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Davis

MICHAEL M. YOSHITSU. *Japan and the San Francisco Peace Settlement*. New York: Columbia University Press. 1983. Pp. viii, 120. \$20.00.

This thin volume by Michael M. Yoshitsu contains insights that many heftier ones lack. The author's skillful reconstruction of postwar Japanese-American relations, based on highly useful personal interviews and hitherto unavailable Japanese-language sources, provides a valuable account of Japan's masterly diplomacy. The result is a revealing story of how the postwar Japanese leadership, so soon after the crippling defeat, was able to manage significant diplomatic successes against heavy odds. The record that emerges demonstrates both Japan's formidable negotiating style and admirable long-range visions that evoke respect.

The central aspect of particular significance is that the Japanese had immediately embarked on elaborate and secret planning to map both the clear

goals and tactics for the diplomatic negotiations to protect their short- and long-term national interests and that this extensive effort was ably assisted by a body of highly dedicated personnel with prescience and premeditation. Yoshitsu reveals that Japan realized well before 1948 that a peace treaty was impossible with the Soviet Union and realistically directed its main efforts toward the United States. After 1949, although some still felt that peace with mainland China was historically desirable, this too was soon abandoned for fear that insistence on this would delay the American treaty and ultimately Japan's own independence.

The real story of Japan's rearmament decisions, however, seems far more complicated than the generally accepted notion that Japan was adamantly against it. Here Yoshida Shigeru's innermost thinking was far different from his public stance, and he was extremely devious and Machiavellian in his secret pursuit of ultimate rearmament. His only concern was how best to maneuver the United States to keep alive the useful myth of Japan's reluctance to rearm. John Foster Dulles was apparently fooled by this ruse and kept up the pressure through 1949 and 1950, well before the Korean War. For instance, on June 22, 1950—three days before the war in Korea—Yoshida was ambiguous and sarcastic responding to Dulles's conclusion that Japan's rearmament was inevitable. With striking resemblance to Adenauer's tactics and coyness, Yoshida circled in parables and said mockingly that Japan's security could be guaranteed by world public opinion, as long as it was "democratic, demilitarized, [and] peace-loving"—near verbatim rehashing of the Allied Powers' early sermons to Japan (pp. 41–42).

With the American demand for guarantees to maintain military bases in Japan, Yoshida blithely countered by proposing an "unarmed Japan" and the neutralization of northern Asia. He was willing to trade off the bases with "unarmed Japan." All along, however, he understood that the treaty terms would have to accommodate closely American wishes, and he was even willing to accept the rearmament level of 50,000—a figure still too low to satisfy the United States.

On China, Yoshida recognized early on that actual relations were not possible. He was determined, however, to conduct diplomacy in such a manner as to lay the foundation for future ties with China, convinced as he was that China and the Soviet Union were not compatible and that China had the ability to ward off permanent foreign influences. Consequently, although Yoshida finally yielded to Dulles's threat that Japan's insistence on ties with China would never be acceptable to the Senate, he continued to hope that some form of relations was possible through the opening of "overseas offices." Ironically, it was this stubborn persistence that led to

a further American request for a written pledge to support Taiwan.

An intriguing question is what indeed motivated Yoshida's indefatigable stubbornness on this issue. Here the answer could be very revealing of Japan's *modus operandi*. The author merely suggests that the reference to the possible opening of "overseas offices" in China was caused by Yoshida's "impulsiveness." But the real motive might have been more deliberate: to remind China of Japan's helplessness under American pressure; to vicariously induce American heavy-handedness for the Taiwan ties, beneficial to Japan's economic recovery while still feigning loyalty to mainland China; and thus to assure China's long-term goodwill.

Even under Dulles's rising pressure, Yoshida again merely promised to have a "bilateral treaty" with Taiwan, not the "bilateral peace treaty"—a very important distinction that Dulles failed to detect. But the most ironic twist of events was that, because of Japanese negotiating team's ignorance of Yoshida's real intentions and desires, Tokyo ended up with a peace treaty with Taiwan. Yoshida apparently could not and did not want to reveal his secrets even to his own diplomats for fear that Dulles would learn of his deceptions. Even worse, the peace treaty that was signed with Taiwan inadvertently expanded the geographic scope of the treaty to the mainland, thus indirectly supporting Taiwan's claim to be the government for all of China.

The main contribution of this pleasant, scholarly work is that Yoshitsu succeeded in making the generally blurred picture of documentary studies bolder and more illuminating. It bares Japan's thinking and attitudes to a degree unattained in other works so that it might even provide an extremely valuable clue to Japan's future attitudes and policies on potentially historic issues—issues such as its eventual policies toward China, the Soviet Union, the United States; trade; defense; and even nuclear armament. This volume is highly instructive on how Japan normally prepares and conducts negotiations and how it sees and goes about attaining its national objectives. In that context, it is a companion volume to Kenneth T. Young's *Negotiating with the Chinese Communists*—essential to those who need to negotiate with the Japanese. The reviewer recommends the book to all those who regard themselves as Japan-watchers, despite the unusual number of insignificant typographical errors and more substantial ones of content, chronology, and misnomers.

T. C. RHEE
University of Dayton

MOHAMMAD ISHAQ KHAN. *Perspectives on Kashmir: Historical Dimensions*. Srinagar, Kashmir: Gulshan. 1983. Pp. 178. Rs. 100.

This collection of six essays by a member of the faculty at Kashmir University is primarily of interest as an example of the central importance Kashmiri intellectuals have given to interpreting their history in relation to their problematic political identity. The work serves well as an introduction to Kashmiri history and to many of the extant sources for that field, sources with which Mohammad Ishaq Khan is clearly at home. It is, however, his interpretations that are of interest and that make this book a primary source in itself. Ishaq's themes are evident throughout the essays, but they are most clearly set out in the introduction, which alone was written specifically for this volume.

Above all, the author insists on the uniqueness of Kashmiri identity, of "a regional and culture personality consciousness" (p. 2) that began in the sultanate period (of the thirteenth to fifteenth centuries) and has, after a long lapse, come to fruition in the late twentieth century. He sees in the early period, above all in its poetry, the hallmark of the *Kashmiriyat* or "Kashmiriness" he takes as his theme. In this period the socially oppressed, he argues, converted to Islam in protest against the domination of high-caste Brahmans. Nonetheless, Hindus continued to develop their culture, both in Sanskrit histories and treatises, and, above all, in the vernacular mystic poetry in celebration of whose values Hindus and Muslims met. Islam, in turn, is unique in Kashmir, he writes, because of its accommodation to the warm, earthy, mystic religion of the local area. *Kashmiriyat* is also characterized by a centuries-old tradition of craftsmen and artists, by food habits, and by a tie to the very soil of the valley. In the last fifty years *Kashmiriyat* has developed a political content based on region, not religion, at a time when elsewhere in India religion became the focus of political allegiance. Today, the author insists, the Kashmiris' national ideal is both Kashmirian and Indian; their international ideal is Islam. Their special autonomy within India must be protected. He opposes Pakistan's holding of "Azad" (free) Kashmir and recalls the war of 1947 as an occasion when Kashmiri Muslims opposed Pakistan.

Not just in such explicit political statements as these, but elsewhere in his interpretations, it is informative to see Ishaq's contributions to an ideology comfortable with the current political situation. He offers support to the Indian nationalist theme of a common culture. He is evenhanded in condemning first the Muslim Mughal rulers as exploiters and oppressors and then the Hindu Dogra Rajputs and their Brahman allies as similarly antagonistic to the indigenous peoples.

Individual essays take up a variety of subjects. An essay on periodization, for example, reiterates the theme of the uniqueness of Kashmir by insisting that measures of the transition from a medieval to a

modern period must be applied regionally. The argument is set in a Marxist framework but looks only at internal class relations, not at larger political and economic systems. A short final essay on the impact of Iran during the late sultanate raises question, also posed in the introduction, of the relation of Kashmir to a larger Central Asian culture. The essays as a whole thus raise suggestive questions and reflect a lively commitment to historic interpretations grounded in political reality.

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ROBERT HEUSSLER. *Completing a Stewardship: The Malayan Civil Service, 1942–1957*. (Contributions in Comparative Colonial Studies, number 15.) Westport, Conn.: Greenwood. 1983. Pp. xvii, 240. \$37.50.

Sequel to an earlier study of the Malayan Civil Service (MCS) until 1942, this volume covers the MCS from 1942 to 1957. These were momentous years for Malaya and also, as their familiar prewar world collapsed, for the MCS officers. With the outbreak of hostilities they suddenly lost their unchallenged role—first to the military and Whitehall, then to Japan; they endured the war in detention in Singapore, working on the Burma-Siam railway, or hiding behind enemy lines. Given recuperative leave at war's end, the survivors saw themselves displaced by military administration men and a whole new cohort of MCS recruits unfamiliar with prewar Malaya, and, amid political upheaval and insurrection, they had to accept and even promote Malaya's evolution toward independence.

Based on personal testimony and case histories, Robert Heussler's study focuses on the "spirit, the values and the working posture of the MCS," the "views and daily work of the men . . . at the center of affairs" (p. 3). A brief, even an apologia, for them, it draws on archives, memoirs, and correspondence to enable the former MCS officers to speak for themselves. This strategy conduces to a particular perspective—a corrective, perhaps, to any facile anticolonialism, but one with its own distortions. Hence, a persistent theme is the contrast between the impractical men of theory in London and the long-apprenticed men of commonsense on the spot.

To make a case for the MCS veterans is one thing; to capitulate to their consoling illusions another. In the accounts on which this narrative is based, commotion is recollected in a brave yet self-regarding tranquillity. As Japanese bombs fall, one officer plays his morning round of golf, perturbed only whether bomb craters are hazards or ground under repair. Asked how he will handle the irresistibly

advancing Japanese, another with similar sangfroid breezily avers that he will simply invite them up for a cup of tea. Once captured in Singapore, the expatriates bear their humiliation with an altogether moving dignity. Regardless of prewar rank, in detention they inevitably formed a rough democracy reminiscent of *The Admirable Crichton*, although the enemy was a different sort of chap: those in authority at the Burma railroad were officious, like jumped-up stewards on country estates back home. Of course, after the Japanese surrender the local populace wanted nothing other than for the old MCS types to return, reassert order, and give a lead. Not steeped in the old ways, however, many in the military administration were boorish men on the make while Britain's Labour government, preferring theoretical abstractions to seasoned commonsense, was addicted to trade union advisers and naive schemes for union development. By contrast, the MCS veterans knew more about Malaya than did their charges: "God gave the Malays a language; Winstedt gave them a grammar" (p. 117). Even on matters of customary law their opinions were accepted unquestioningly, the Malays knowing little of such matters and being little interested to learn. Such men, naturally, neither felt nor were seen by the locals as aliens; thus when, as independence approached, well-placed Malays who knew English chose instead to speak Malay, MCS veterans could confidently recognize this preference as simply an attempt to feel superior.

For Heussler, British rule in Malaya was a triumph of responsible stewardship by tried, true, objective, and ever-benign paternalists. The nostalgia for empire that suffuses his study is understandable coming from the British and especially from their last colonial pensioners. That the United States, yet really to acknowledge its own imperium, can produce it is disconcerting.

CLIVE S. KESSLER
University of New South Wales

GORDON REID. *A Nest of Hornets: The Massacre of the Fraser Family at Hornet Bank Station, Central Queensland, 1857, and Related Events*. New York: Oxford University Press. 1982. Pp. xiii, 235. \$36.95.

In November 1857 a group of Aborigines attacked an outlying sheep station in central Queensland, Australia, killing eleven adults and children and raping three grown women. Versions of the massacre of the Fraser family and its aftermath have become part of white Queensland folklore, with the surviving Fraser brothers' fierce revenge taking on something of the status of Ned Kelly's exploits in the South. Gordon Reid has made this tragedy the center of a study of race relations on the Queens-

land frontier, in which he attempts to sort fact from fiction and to relate both to government policy.

The writing of the history of the relations between indigenous inhabitants and European invaders is a younger enterprise in Australia than in America. Only a few path-breaking general studies have been published, and most of the completed regional studies remain in thesis form. The novelty of Reid's study lies mainly in its approach and focus; its general conclusions do not depart radically from those of forerunners like Henry Reynolds and David Denholm.

Reid's approach to his sources is admirably exhaustive. Every available scrap of contemporary evidence, every retrospective account, every oral reminiscence is discovered and discussed. The great majority of his witnesses are necessarily European; the Jiman aborigines have been so thoroughly slaughtered and dispossessed that no one claiming that ancestry remains alive today, and Reid is by no means certain that he has correctly named them. But he has searched hard to find oral aboriginal testimony and to disinter aboriginal voices from contemporary European records.

Unfortunately this virtue in research becomes a liability in the text. Reid here is too much historian and too little novelist. Too often the minutiae of evidence overwhelms his narrative, as Reid pauses to argue with his witnesses. His determination to confront the folk myth of innocent dead and just avengers means that the myth often structures the whole—and yet the myth is never fully told. For non-Queensland readers, a useful literary device may well have been to begin with a full account of the myth—and of its significance in the Queensland consciousness—before attempting its deconstruction.

Reid is committed to reconstructing "what actually happened" on both sides of the frontier, to showing Aboriginal motivation as well as European. Few historians will quarrel with his central thesis that the massacre was an almost inevitable result of the European expropriation of aboriginal land. But some will take issue with his conclusion that the attack was part of a carefully orchestrated resistance, that the Jiman and their allies chose to "use a new technique, commonly used by modern terrorists: they would commit outrages so horrifying to the invaders that they would withdraw" (p. 181). The conclusion is unverifiable on the evidence, but it should at least clarify debate.

By contrast, Reid's concluding account of European attitudes is fully convincing. He gives new symbolic value to the surviving Frasers, presenting them almost as victims—failures as landowners, doomed at least partly by their obsession with revenge to remain laborers for the successful men in whose interests their mother and siblings died—powerful

men whose opinions and needs have shaped present-day Queensland. In Reid's new reading, "By the end of the 19th century, white society in Queensland had trapped itself in a form of racism born out of the conquests of the past, the social relations they produced, and the egocentricity of Australian isolation" (p. 189).

MARIAN AVELING
Monash University

UNITED STATES

HENRY F. DOBYNS. *Their Number Become Thinned: Native American Population Dynamics in Eastern North America*. Assisted by WILLIAM R. SWAGERTY. (Native American Historic Demography Series.) Knoxville: University of Tennessee Press, with the Newberry Library Center for the History of the American Indian, Chicago. 1983. Pp. 378. Cloth \$29.95, paper \$14.95.

The size and complexity of Indian populations when Europeans first arrived have usually been grossly underestimated. Before the earliest useful counts were recorded anywhere in the New World, introduced diseases to which the Indians lacked resistance had much more disastrous effects than previously supposed, both where Europeans were already present but left few relevant documents, and in vast areas well beyond the frontiers of European settlement and exploration. Low estimates probably also reflect prejudice, for they have long helped to justify the European conquest of America. In a new climate of opinion, Henry F. Dobyns in previous studies emphasized revisionist interpretations, extrapolating to whole continents the depopulation ratios worked out for California and Mexico by S. F. Cook and Woodrow Borah. In this book he turns to a detailed examination of the size, and some of the distribution, of the aboriginal population of Florida up to its final extinction in 1763, justifying his broader title by adding a brief consideration of some New York Seneca archaeological data.

In the absence of censuses, records of individual births and deaths, convincing counts of any single settlement, or even completely excavated cemeteries, indirect evidence must be used. The recipe goes as follows.

1. Estimate the carrying capacity of the whole state. Accept all guesses and ignore contrary evidence to decide which plants and animals were eaten. Determine their dietary yields from modern estimates, often for quite different species. Judge their distributions and density, and the harvesting rates compatible with maintaining the populations

of these resources, by generalizing to the whole state from modern data on restricted local environments. Gauge the efficiency of Indian subsistence strategies by choosing the most optimistic interpretations, and by extending agriculture beyond its known limits (both geographical and in plants cultivated). State the results in grams per person, to two decimal places.

2. Assume that when Europeans arrived, Indian populations had reached the maximum size possible under the above conditions.

3. Accept the highest of the occasional comments by early observers on the size of villages, the occupancy of houses, the population of small regions, the number of warriors on specific occasions, and the like, and choose multipliers that will yield the largest total population. Adopt the round-number estimates in the sources, including those attributed to Indians, as if they reflected essentially modern attitudes toward enumeration, and do not consider the possibility of self-serving bias in early reports. Overlook the striking disparities common even in modern estimates, such as those of the sizes of crowds of demonstrators in the United States during the 1960s and early 1970s.

4. Generalize the scattered archaeological evidence on settlement size and density, downgrading environmental and cultural differences.

5. Draw comparisons and analogies with regions that were culturally and environmentally very different from Florida, such as Lower California, Arizona, the Andes, and central Mexico.

6. Use all isolated hints for the occurrence of epidemics, assuming undocumented routes and rapidity of spreads. Guess the diseases involved, and determine mortality rates by analogy from other regions and periods. Multiply to cover all of Florida, and extrapolate to get curves of population totals.

The result is an estimate for the Florida population at about A.D. 1500 that is nine times larger than the best recent one. The evidence is much more extensive than ever previously marshaled, but the outcome is unconvincing. Fortunately Dobyns is quite explicit about his methods: far-fetched analogies, circular reasoning, leaps in logic, and the multiplying of guess by guess are easily recognized. Signals such as "probably," "surely," and "could have" are frequent. The author does not obfuscate with statistics, for he uses no mathematics beyond arithmetic. Thus a careful reader quickly perceives how rickety is the structure Dobyns has erected. There is, however, often even less basis for his hypotheses than meets the eye, for Dobyns tends to ignore, rather than discuss, contrary evidence and interpretations both in the sources he cites and in those he does not. Systematic bias in interpretation is frequent.

The population estimates here are among those

at the impossibly high upper limits of the range of estimates one can cite to indicate that there is as yet no reasonable answer to the question, "How many Indians were there in 1492?" If that were all, little harm would have been done. The danger is that this book will be referred to for other things, such as its summaries of Indian subsistence techniques and sociopolitical systems, the map of Timucua chiefdoms, the population subtotals, and the list of epidemics. On these matters the only redeeming value of the work is that it makes explicit many problems—some of them only seeming problems—that must now be resolved. There is much here that will unfortunately have to be corrected by subsequent writers on Florida Indian ethnohistory.

WILLIAM C. STURTEVANT
Smithsonian Institution

FRED HOBSON. *Tell About the South: The Southern Rage To Explain*. (Southern Literary Studies.) Baton Rouge: Louisiana State University Press. 1983. Pp. xii, 391. Cloth \$35.00, paper \$12.95

Southern sectional consciousness dates from the seventeenth century, but the sectionalism that has been a distinguishing feature of the region emerged only with the Missouri controversy. In the 1840s and 1850s the attack on slavery questioned all aspects of southern life and was met by a corresponding defense. From that time until the 1970s two general schools emerged within the South, one that was critical and tended to rely on historical contentions and another that was apologetic and used romantic and poetic arguments.

Tell About the South investigates the leading non-novelist southern writers (and the author contends nonscholars as well, although a number of his selections belie this) who wrote from the 1850s, when the concept of a southern nation emerged, until the 1970s when the South assumed a new stance within the nation. Roughly they may be classified into two groups: the Defenders of Slavery and School of Remembrance, which includes James H. Hammond, George Fitzhugh, Edmund Ruffin, Edward A. Pollard, Robert Lewis Dabney, Thomas Nelson Page, Donald Davidson, William Alexander Percy, Ben Robertson, and Richard M. Weaver; and, second, the School of Shame and Guilt, which includes Hinton Rowan Helper, Daniel R. Hundley (in some aspects of his work), George W. Cable, Lewis H. Blair, Howard W. Odum, W. J. Cash, Ralph McGill, Pat Watters, and Lillian Smith. James McBride Dabbs is presented as a complex figure whose works are related to both schools.

Of necessity, Fred Hobson relies heavily on published works dealing with the authors he investigates, although he also uses a commendable quanti-

ty of manuscript materials. Little that was previously unknown about the authors is presented, yet *Tell About the South* is a valuable and significant work. Interesting comparisons are made, demonstrating the continuity that existed over the years among writers in the two major schools. The general student will use the work as a convenient compendium of information about the authors and a guide to more detailed studies of them. The specialist will find intriguing insights that, in some cases, will be disputed but will serve as a stimulus for additional investigation and evaluation. For example, in his excellent analysis of the conflict between the agrarians and the liberals, Hobson contends that much of W. J. Cash's disagreement with the agrarians stemmed from a conflicting idea of culture, which the agrarians regarded "as an organic expression of the folk" (p. 266). He also holds Howard W. Odum was "not even first and foremost a realistic social scientist" (p. 200), and he finds Donald Davidson's commitment, like Edmund Ruffin's and Robert Lewis Dabney's, to be greatest when he felt rejected by his fellow southerners (p. 220). He believes Walker Percy to be "the sanest and most acute observer of the contemporary South" (p. 290), the South itself to be "more concerned with power and public relations than the burden or glory of its past" (p. 352) and that it has assumed a role of "moral superiority" in the nation (p. 354).

HUGH C. BAILEY
Valdosta State College

JESSIE POESCH. *The Art of the Old South: Painting, Sculpture, Architecture, and the Products of Craftsmen.* (Borzo.) New York: Alfred A. Knopf. 1983. Pp. xii, 384. \$50.00.

Encompassing three centuries of antebellum history, *The Art of the Old South* includes within its broad purview painting, architecture, sculpture, and the crafts, as well as insights into the lives of those who made this art possible. All sorts of scholarly perils surround such an ambitious study, but Jessie Poesch has managed to steer a careful and largely successful course through dangerous waters. If her achievement is a coffee-table book in size and lavishness of illustration, it does not merit the pejorative connotation of the term. Far from superficial, the work derives from an impressive array of fresh primary and secondary sources, and the author's personal and incisive analysis of the art and artifacts under consideration.

Poesch has posited no new or radical thesis. Rather, she has offered a solid historical survey of subjects that hitherto generally have received fragmented coverage through monographs. What most clearly emerges from her work is a sense of the rich

diversity of artistic creation found in the Old South. And how could it have been otherwise, one wonders, for the region included disparate cities, towns, countryside, and frontier and drew on motley foreign influences: English, Scotch-Irish, French, Spanish, African, German, Swiss. Its culture, moreover, reflected the needs and wants of the several social classes, changing tastes, and particular public and private purposes. Then, too, there was the peculiar genius of the individual. Names of familiar artists resonate throughout the book—John James Audubon, Benjamin Henry Latrobe, members of the Peale family, for example—but no more so than those of hundreds of lesser figures such as the Charleston cabinetmaker of the Revolutionary period, Thomas Elfe, or the versatile nineteenth-century painter, George Cooke. The author, by defining the boundaries of southern culture so inclusively, has conveyed most convincingly a feeling for the region's artistic diversification and provided the reader with an abundance of interesting if sometimes arcane knowledge.

For all its strengths *The Art of the Old South* exhibits weaknesses as well. In her laudable attempt not to omit anything of importance, the author occasionally leaves the nonspecialist entangled in a thicket of facts. Relatedly, the chronology of the work flows smoothly enough, but abrupt transitions from genre to genre or person to person make the thread of the narrative at times difficult to follow. Also, there is no serious attempt to explain how or why or when the art of the Old South did differ—when it substantially differed at all—from that of other regions, although, in all fairness, this is a complex topic ultimately fit for another kind of study. These few reservations notwithstanding, the author deserves high praise for having given suitable form to what otherwise could have been an uncritical or inchoate exposition of an important subject.

ROBERT MUCCIGROSSO
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DAVID G. PUGH. *Sons of Liberty: The Masculine Mind in Nineteenth-Century America.* (Contributions in American Studies, number 68.) Westport, Conn.: Greenwood. 1983. Pp. xxii, 186. \$27.95.

David G. Pugh presents his work as a venture into "cultural criticism," an attempt, from both psychological and historical groundings, to build "integrated knowledge and synthesis" about the "masculinity cult in American life." Starting with the assumption that "manliness" or "masculinity" is a complex, psychosexual state of being, he finds its first "official prototype" in the United States in the image of

rough, ready, and antiaristocratic President Andrew Jackson. The "masculinity cult" that then formed, he argues, has had a pervasive influence, especially in popular culture.

This is old wine in a new bottle. Pugh has adopted some of the ruling notions of historians and psychologists from the late 1950s to the early 1970s about "American" character, and renamed them (accurately enough) masculine. He has added virtually no research into nineteenth-century sources of his own, except for dips into the fiction of James Fenimore Cooper, Nathaniel Hawthorne, or Henry James, and resort to the social commentary of Alexis de Tocqueville or Thorstein Veblen. Nor is Pugh up to date in the stringing together of secondary sources that constitute his findings. He cites nothing published after 1976, and relies so heavily on earlier books that the reader senses that *Sons of Liberty* was meant to be published a decade ago. Thus, on American (male) character, particularly regarding projection of male anxieties onto female figures, our guides are Geoffrey Gorer and Erik Erikson; on the Jacksonian mystique, the findings are Marvin Meyers's, John William Ward's, and Richard Slotkin's; Leslie Fiedler's ideas shape the readings of imaginative literature; on women's roles, Kathryn Kish Sklar, Barbara Welter, and Ben Barker-Benfield carry the day; on the post-Civil War era, Richard Hofstadter's, Robert Wiebe's, and Thomas Cochran's works inform of changes in views of masculinity; and on twentieth-century "he-man" magazines (where Pugh does some original research) John Cawelti's analysis rules.

Pugh has the good sense to recognize that previous characterizations of the "American" pertain mainly to the white male American (and may thus indicate archetypes of gender and race as much as national character). He does not apply the same good sense, unfortunately, to the problem of abstraction epitomized in his title, "the masculine mind." Throughout, the reader is left to wonder who manifested or practiced the "masculinity" described. As the closing chapter moves to the "super-male" image in "macho" magazines (*Stag*, *Climax*, *True*, and *Argosy*) of 1957–75, this question becomes all the more insistent, since the resonance of such instruments of popular culture is hardly established. Pugh has identified an important area for historical investigation, but his methods are inadequate to the task.

NANCY F. COTT
Yale University

MARILYN FERRIS MOTZ. *True Sisterhood: Michigan Women and Their Kin, 1820–1920*. (SUNY Series in American Social History.) Albany: State University of New York Press. 1983. Pp. xii, 199.

This careful monograph uses letters of white, Protestant, native-born women who lived in Michigan between 1820 and 1920 to support the thesis that it was the female-centered kin network, not the relatively fragile and unstable nuclear family, that provided women with emotional and economic support in a hostile world. Chapters discuss motherless children, husbands and wives, widows, and nurses—the last considered a paradigm of nineteenth-century domestic ambivalence, in which women were "powerful while powerless, controlling while controlled" (p. 110).

The letters, which are analyzed in context in terms of "male" and "female" language (not a wholly satisfactory historical exercise in this case), show few influences from institutions or events. Changes in technology, whether they affected transportation, dressmaking, or family planning, seem to have gone unnoticed, not to mention changes in religion, mores, or war. Although appendix B gives a brief dry summary of Michigan history, offered presumably because most historical studies ignore the Northwest Territory after 1785, it leaves out most of the changes that would affect women's lives. The University of Michigan, elsewhere considered such an impressive leader in women's education, is here unsung. Even when photographs are provided, there is little attempt to differentiate one woman's correspondence from another, and since chronology is not emphasized, and several generations are frequently cited, the writers tend to blur into a long undifferentiated continuum of networking. For the purposes of this book it is clear that the individual woman is considered much less important than the kinship pattern into which she fitted, but it is not necessary to so arbitrarily reduce her to a spoke on a genealogical wheel.

Apparently the sources are as immune from internal dissension as they are from external commentary. No venality over the disposition of mother's antimacassars, no snide comments about unloved or unlovely second cousins, not even recriminations against the traditional enemy, the mother-in-law, show rifts in the family lute. Marilyn Ferris Motz warns against romanticizing those halcyon days, since they were the product of desperate fear, anxiety, and helplessness, rather than a positive inherent family strength. Access to work and education largely remove the need for the female family as haven, a conclusion not completely hopeful insofar as contemporary "sisterhood" is concerned.

Even as the impressive emphasis of female solidarity and love that literally was believed to last beyond death accumulate, the reader wishes for some individual who broke out of this admirable pattern—some rebel daughter longing to do something different, even something as tame as being a

missionary, some negative, mother-hating ingrate, even some prototype of Jennie Gerhardt. The writer concludes that woman's power base consisted almost completely in "manipulation of the interaction among family members" (p. 5), an ambition now more worthily displayed in the boardroom and the classroom. Yet if women manipulated their families, there should be negative, as well as positive, evidence—evidence that would go a long way toward eliminating the sense of triviality, and tedium, that the letters present. A final warning to the reader might also be, in the tradition of Sylvia Plath's *Letters Home*, to resist uncritical acceptance of the family letter as unvarnished historical truth.

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LOUIS B. WEEKS. *Kentucky Presbyterians*. (Presbyterian Historical Society Publications.) Atlanta: John Knox. 1983. Pp. ix, 190.

Within the last two decades the religious history of the South has become a fertile field of scholarship. There have been regional histories; histories of particular denominations; biographies of church leaders; local studies of churches; and analyses of specific movements, events, or controversies. In the past much that was written on any of these topics was narrowly denominational or filiopietistic; now the tools and viewpoints of social, cultural, and intellectual history predominate. Yet most of the newer accounts still depend on older studies that are defensive in tone and provincial in outlook. Historians with broader views and more sophisticated methodologies need to turn their attention to such topics as particular denominations in a single state; from these detailed investigations scholars will, for example, deepen their understanding of the regional variation within southern religion.

Louis B. Weeks has sought to provide one such modern state study. It is appropriate that the Presbyterians of Kentucky are his subject, for they have played a disproportionately influential role in the written history of the South. From the emancipationism of David Rice to the controversy over Transylvania University to the career of Robert J. Breckinridge, even the nonspecialist is familiar with some aspects of the topic. What Weeks has done is provide a judicious narrative of the Presbyterian experience in Kentucky from the founding to the present, with most attention lavished on the period before 1865. The difficulties of the Civil War era are especially well covered. The factual data from earlier accounts are put in a broader context by the

author's utilization of recent scholarly studies. Weeks is interested primarily in institutional history, and the activities of churches, synods, and ministerial leaders dominate his account. In this sense the book is less up-to-date in approach than some might have wished.

Influential men and organizations are obviously important, but so too are ideas, both formal theology and the religious beliefs of the common people. Women usually outnumbered men in the churches, but their activities are inadequately treated here. Blacks, free and slave, also attended Presbyterian churches, but their role is slighted except in the context of the abolitionist sentiments of several of the early church leaders. The religious culture that was expressed through Presbyterian church life and home devotions is hardly discussed. Had Weeks chosen to write more social history and less church history, his book would have been more valuable. As it is, *Kentucky Presbyterians* is a workmanlike study, less provocative than balanced, less interpretative than narrative. Rather than expand the specialist's knowledge it largely confirms what was known, but it does so carefully, clearly, and accurately. That is no mean achievement.

JOHN B. BOLES
Rice University

PETER GROSE. *Israel in the Mind of America*. (Borzoi.) New York: Alfred A. Knopf. 1983. Pp. xiv, 361. \$17.95.

There is much to enjoy in Peter Grose's history of the role played by America and its leaders in the creation of a modern Jewish nation. His scope is ambitious and the results are by and large successful. The author traces 150 years of interest and involvement in behalf of a Jewish homeland among both gentiles and Jews in America; he covers with particular scrutiny the lobbying campaign of American Zionists that culminated in 1948 with dramatic and significant United States support for newly created Israel.

Because of both the period of time covered and the number of separate topics pursued, this volume does not provide an exhaustive study of any particular aspect of the material handled. It is instead a fresh examination of both the previously available and the newly released historical documentation in a competent synthesis of the issues involving America and the movement to create a Jewish home. There is no major new interpretation or revision of the manner in which previous scholars in the field have handled key controversies. One strength is the author's use of a wide range of government documents, including material derived from the archives

that only now has been made available by the United States, Britain, and Israel.

Another positive aspect is Grose's clear, readable prose. His years as a foreign correspondent for the *New York Times* apparently served him well in developing a style that will make this carefully documented book appealing to the general reader as well as the specialist. Particularly noteworthy are the excellent biographical sketches of the major characters whose careers entwined with the major controversies—men such as Louis Brandeis, Stephen Wise, Abba Hillel Silver, Loy Henderson, and Breckinridge Long. Grose does well, too, in delineating the distinction that has always existed between American Zionists and those from Europe. American Zionists never felt they needed Zionism for themselves but rather adopted a Zionism that was philanthropic in nature. The distinction continues today. Whereas Israel is of importance in the lives of American Jews, the Jewish state and its existence is the central preoccupation in the lives of Israelis.

There are some cautionary flags that have to be raised in evaluating this work. The author clearly sympathizes with the Zionist cause over that of the Arabs, and sometimes this prejudice intrudes into his analysis of the material. Public figures supportive of the Zionist program are treated more kindly than those who sided with Arab claims to Palestine. For example, when General John Hildring secretly worked on contingency plans to transport Jewish refugees to Palestine, Grose refers to "a compassionate general in the Pentagon" (p. 205). Never are proponents of a pro-Arab policy for Palestine referred to in such terms. Missing in this volume is an acknowledgment that the Palestinian Arabs had a reasonable claim to Palestine, and that support for their position could involve just as much compassion as could a position supporting Zionist goals.

All too often and too easily the author attributes Arab support to the anti-Semitism of the supporter. There is no doubt that anti-Semitism existed; in fact, Grose does a thorough job in identifying the tradition of anti-Semitism that had long existed within a certain "patrician element" of State Department employees. It is unfair and inaccurate, however, to assume that American officials who opposed Zionism (whose basic program advocated a Jewish state and no Arab state in Palestine) could have arrived at that position only out of an anti-Semitic sentiment.

Potential negative strategic consequences, fear of jeopardizing a fuel source, risks in alienating an Arab world that had strong diplomatic ties to the United States, and compassion for Palestinian Arab claims to their homeland all provide an alternative way of approaching the issue that led many to differ on quite reasonable grounds with the supporters of Zionism. Although anti-Semitism was the motivating reason for some, the overwhelming opposition to

Zionism from virtually the entire foreign policy community in Washington basically derived from an analysis motivated by national security concerns that it would be detrimental to America to create a Jewish state in Palestine. Grose's interpretation would have been stronger had he recognized this.

It is unfortunate that Grose uncritically evaluates presidential aide Clark Clifford's explanation concerning President Harry Truman's recognition of Israel just eleven minutes after it came into existence. Clifford has tried to resurrect Truman's status as a statesman during this episode by suggesting that Truman's immediate recognition was not done *solely* for the politically expedient reason of trying to win Jewish votes during an election. Clifford has indicated that an additional reason for Truman's sudden recognition derived from his statesmanlike decision to recognize Israel before the Soviet Union. The catch in this argument is that there is no logical reason why being first would do anything to enhance American security or work to our national benefit. The only advantage to beating the Soviets in recognizing Israel would be to add to Truman's support among approving Jewish voters.

JOHN SNETSINGER

California Polytechnic State University

ROBERT J. WILSON III. *The Benevolent Deity: Ebenezer Gay and the Rise of Rational Religion in New England, 1696–1787*. Philadelphia: University of Pennsylvania Press. 1984. Pp. xv, 311. \$25.00.

This account of Ebenezer Gay is at once the finest study of American Protestant liberalism since W. R. Hutchison's *Modernist Impulse* (1976) and one of the most impressive of recent efforts to integrate intellectual history with social context. Product of a Harvard increasingly cordial to influences of the early English Enlightenment, Gay, in 1718, began a career as pastor and first citizen of Hingham that was to span nearly seven decades. At first a cautious advocate of evangelical revival (he opened his pulpit to Gilbert Tennent in 1739), Gay was provoked by the multiplying excesses of the Great Awakening in the early 1740s to take up the cry against "enthusiasm." The Great Awakening also hastened his turn, as did his continuous consumption of English latitudinarian theology and the works of Hugo Grotius, to an openly Arminian conception of the Christian redemption. Under Gay's leadership the South Shore region emerged as the most organized bastion of liberal strength. Although in immediate reaction to the Great Awakening, liberals joined with Old Light forces in opposition to the New Lights, by mid-century Gay, his colleagues, and their congregations were emerging as a separate and militant party squarely at odds with their Reformed

heritage. Robert J. Wilson III draws from archival sources an admirably detailed account of the ecclesiastical maneuvering that accompanied this development, illustrating again and again the importance of Gay's leadership. In Hingham itself, Gay employed his forceful personality, his leading position within the ruling oligarchy, and his immensely long term of office to become virtually the town's living embodiment—a circumstance that also makes it easier to grasp the sociological connections of the Arminian theology. For Gay's career in Hingham coincided roughly with an advanced stage of the town's commercialization and the rise to social and political dominance of its mercantile elements. In this development he was both advocate and participant. His theology of human freedom and moral ability spoke for and to the interest in personal autonomy and aspiration that was coming to the fore; it meshed with the sense of confidence and well-being natural to a period of economic growth; and its stress on moral responsibility provided a comforting formula for social stability in a more individualistic society.

Wilson's narrative is not always theologically precise. He interprets Gay's emphasis on repeated and deathbed repentance, an English Reformed commonplace for nearly two centuries, as a repudiation of "perseverance of the saints"; he appears to regard "preparation for conversion," a procedure affirmed by all Puritan predestinarians including William Perkins, as a breach of "strict Calvinism" (p. 24). More seriously, he tends to measure Gay's Arminian turn by the standard of a model of Reformed theology in which human agency and ethical responsibility is minimized if not excluded. The relevant distinctions are subtler than one can learn here. For intellectual historians the value of this otherwise excellent volume would have been enhanced by a separate chapter or section displaying the theological issues in more focused and systematic detail. Yet on the whole this book is an admirable performance. It will stand long as the definitive treatment of Gay's life and mind.

T. DWIGHT BOZEMAN
University of Iowa

LYNNE WITHEY. *Urban Growth in Colonial Rhode Island: Newport and Providence in the Eighteenth Century*. Albany: State University of New York Press. 1984. Pp. xiv, 183.

Complaints about the proliferation of colonial community studies are fashionable: unsympathetic reviewers frequently call them formulaic or tiresome and accuse their authors of Balkanizing early American history. In reality, the complaints are more repetitive than the case studies: the writing of local history is in a constant process of modification and,

as with most new genres, is becoming more sophisticated as it develops a sizable *corpus* of materials. It Balkanizes the past no more so, and perhaps less, than biographies or accounts of celebrated events. And, it is widening its scope to ask new questions about little-studied places or types of places.

Much of the first wave of community studies was about Puritan New England and was concerned with questions of political leadership, democracy, stability, and harmony. Much of the second wave concentrated on the Chesapeake region and asked questions about social and economic relationships. Ripples from both waves have now reached most areas of the colonial world including the shores of Narragansett Bay. Lynne Withey's comparison of urban development in Newport and Providence, Rhode Island, is a fine example of the growing maturity of local history. Informative and charming, it charts the economic growth of the two cities, the reasons for the growth, and the effects of it on their social structures. The basic outlines of Withey's story are well known: Newport rose to become New England's second-most important trade center in the middle third of the eighteenth century; Providence began its urban development later, pulled even with Newport on the eve of the Revolution, and then replaced it as Rhode Island's leading city in the two decades following the Revolution. The causes and social manifestations of this growth, however, have been previously unstudied. Withey shows that differing patterns of development created differing elites, networks of trade, and rates and types of population increase in the two rivals. Despite these differences both cities experienced some problems in common: an inability to escape the dominance of Boston and New York; a burdensome increase in the need for poor relief; and increasing social tensions occasioned by a growing maldistribution of wealth. To substantiate all of this, Withey assembles an impressive array of quantitative materials; she does not, however, allow this data to tyrannize her prose. Rather than providing an extended commentary on tables and charts, she tells a story that commands interest and relegates most of the data to appendices.

For the most part, Withey's work is careful and convincing. Not surprisingly, I have my quibbles, most of which are concerned not with what she has done but what she did not do. More explicit comparisons with Connecticut's five urban centers that were similar in size and function would have provided a richer context; a discussion of local politics, which she almost completely ignores, would have helped sharpen the focus; and more research in the colony archives would have revealed additional tensions within and between the two cities. *Urban Growth in Colonial Rhode Island* is limited in scope; yet, it tells us much about why and how two Rhode

Island towns grew from villages to cities and it is an important contribution to Rhode Island, urban, and social history.

BRUCE C. DANIELS
University of Winnipeg

MARY LOU LUSTIG. *Robert Hunter, 1666–1734: New York's Augustan Statesman*. Syracuse: Syracuse University Press. 1983. Pp. xvi, 277. \$32.00.

One of Scotland's most important exports to England has been a steady stream of poor, young men who travel south to make their fortunes among the "sassenachs." Robert Hunter was one such youth and where many failed he succeeded, for by the time he died he had capped a successful army career with appointments as governor of New York, New Jersey, and Jamaica. While his career was one of the most notable in the eighteenth-century British empire he has lacked a biographer. Mary Lou Lustig has now provided us with an able and comprehensive account of his life.

Hunter's career was the usual eighteenth-century meld of talent coming into contact with opportunity, combined with the all-important patronage of great men. As a soldier he advanced himself during service in Europe and in Scotland, where he repressed his fellow Scots. He came to the attention of the Duke of Marlborough and other military leaders of Whiggish disposition and prospered until he appeared to push himself ahead of one of the duke's favorites. Shortly after this episode he retired to England to find solace in a brilliant marriage to the wealthy widow of a brother officer. Away from the wars he could exercise his considerable nonmilitary talents among London's literary lions. Swift, Steele, and Addison were his friends and he contributed to the *Tatler*.

But Hunter always had a vigorous political side to his nature and he sought to further his career by acquiring a position in the American colonies—a favorite reward for military men. In the end he was appointed governor of New York and New Jersey. He proved to be a skillful administrator and politician who tamped down the dying embers of Leisler's Rebellion and created a "faction" of his own in New York, and when he retired to England after nine years in office he could survey a province at peace and content. He was not quite ready to retire so he returned to the colonies to govern Jamaica. This term in office proved to be less successful. Hunter wrestled with the assembly over a number of issues but little was resolved by the time he died in 1734.

Lustig has crafted a thoroughly researched and exemplary biography of an outstanding imperial politician. She is particularly good at describing the interplay of English and colonial politics that placed

Hunter on shaky grounds on more than one occasion as enemies at home and in the colonies sought to undermine his position. Although he survived these assaults Lustig shows just how much effort it took on his part to maintain his position in the ever-changing currents of imperial politics. When it comes to Hunter's opponents in the colonies Lustig recognizes that Hunter was quite willing to use accusations of treason, sedition, and Jacobitism to attack the members of the assemblies who opposed him. But she also sees local politicians from over Hunter's shoulders as meddling and interfering sorts who should have obeyed the royal prerogative and who did not because they were "ruled by emotion" (p. 212). Although local politicians might not have measured up to Hunter's standards of probity they were political men fighting to advance local interests and the rights of the assemblies, particularly over money matters. Their stubbornness, although offensive to Hunter, furthered the cause of legislative independence in America. Nor does she see beyond these politicians to the electorates behind them. Hunter built his own faction in New York but we never learn what groups in the electorate favored his cause. He sided with the great landowners of New York who voted with him in the assembly, but who voted for the landowners? This is, in other words, a biography written from Hunter's perspective and as a defense of an admirable man. In its scope and concerns it is surely a definitive study.

ROBERT C. RITCHIE
*University of California,
San Diego*

RONALD HOFFMAN and PETER J. ALBERT, editors. *Arms and Independence: The Military Character of the American Revolution*. (Perspectives on the American Revolution.) Charlottesville: University Press of Virginia, for the United States Capitol Historical Society, Washington, D.C. 1984. Pp. xii, 243. \$20.00.

This volume consists of nine essays read at the fourth symposium on the War of Independence sponsored by the United States Capitol Historical Society. The fact that only half of the published essays have invited comment is a reflection, not of the quality of the rest, but of the necessary limitations of space.

Don Higginbotham's "Reflections on the War of Independence" is set in the context of the Vietnam War when, for the first time, America's Revolution seemed to have a latter-day relevance. Higginbotham warns that analogies can be overdrawn and simplistic but he points to several similarities. As with Americans in the 1960s, Britain suffered "from

a superpower mentality, from a conviction that she could not lose a war" (p. 13). Both Britain and the United States hoped to create an infrastructure of local inhabitants that would provide the basis for a pacification program. Instead, in each case, the program failed because a savage guerrilla war of reprisal made attempts to pacify the dissidents impossible.

Charles Royster believes that in the consolidation of the nation after 1783 a principal element was "Founding the Nation in Blood." Patriotic orators held forth "in extended, lurid detail on the bloody deaths of patriots or enemies" (p. 35). Without arguing with Royster's central point that the military tradition was an important element in the development of a national consciousness, it is suggested that the collective memory more often called up the panoply of war, banners whipping in the breeze, the long ranks of uniformed men on parade, rather than heaps of grisly corpses.

In "Samson Shorn," Richard Buel points out that Revolutionary leaders vastly overestimated American manpower resources. After the patriotic fervor of the early months of the war had cooled, Congress found that not only was the expense of maintaining the army enormous but that ranks could be filled only by offering generous bounties. This put an unbearable strain on the congressional purse. The deterioration of its financial resources was more complex than the simplistic notion that Congress flooded the country with worthless paper money. The American agricultural economy produced surpluses of farm commodities that were traded for imported manufactured goods. One of the effects of the British blockade (little noted by most historians) was to reduce drastically the supply of foreign goods, thereby destroying the farmers' incentive to produce surpluses. This disruption of the American economy would have seriously inhibited the ability of Congress to support the army even if it had been able to levy taxes and stabilize the currency.

In the final essay, "The Military Significance of the Revolution," Theodore Ropp concludes, "We still have difficulty in connecting its great political and minor military significance" (p. 230). He believes that military historians have paid too much attention to the military means by which Americans achieved independence and too little to the fact that it was essentially a political war fought to achieve important—and limited—political ends.

All of these essays exhibit the two outstanding qualities of historic endeavor: sound scholarship and provocative conclusions that provide much food for thought and that will direct many of us down the roads that we might not otherwise have traveled.

JOHN PANCAKE
University of Alabama

EUGENE R. FINGERHUT. *Survivor: Cadwallader Colden II in Revolutionary America*. Washington: University Press of America. 1983. Pp. 194. Cloth \$21.75, paper \$10.50.

This biography of the second son and namesake of the contentious crown official and last royal lieutenant-governor of New York blends narrative with analysis to examine the rise of egalitarianism and the constraints inhibiting social change in Revolutionary New York.

Born in 1722 and raised on his father's Ulster County estate, "Coldenham," Cadwallader Colden II became a successful farmer and marginal political figure. His marriage in 1745 to Elizabeth Ellison was a shrewd economic move and a practical matter—"I thought it was proper to look for a housekeeper before my house was finished," he later recalled—but for more than a half-century his family was the source of stability in his life. Eight of his twelve children survived to adulthood. Two sons fought for the British in the War for Independence, another was an inactive Whig, and two others avoided involvement altogether. Colden's Ellison in-laws supported the Revolution and helped Mrs. Colden keep their estate during the war. Colden tried to convince New York officials in 1777 that he was a "faithful and true subject of that State from which he receiv'd protection," but that because of his "oath of allegiance . . . to the British king" he ought to be "permitted to observe a state of neutrality" (pp. 58, 84–85). The machinery of Revolutionary justice moved haltingly to confront this novel interpretation of allegiance, finally forcing him into exile in New York City. The garrison-town atmosphere there energized him. He parlayed his famous name into selection as president of a group of "Loyal Refugees" and vainly sought a crown appointment.

But he did not depart with the British in 1783. "In a region rife with guerrilla warfare and conflicting allegiances," Eugene R. Fingerhut concludes, "he was reticent and feared precipitous action. These were his saving characteristics" (p. 117). That pithy evaluation is a key to understanding Colden and the environment in which he managed to survive the Revolution. His lack of counter-Revolutionary zeal and his inarticulate, stolid approach to politics shielded him from the wrath of the patriots; loss of social eminence may well have aided his post-Revolutionary attempts to recover, out of the proceeds of confiscated estates, money owed him by loyalist exiles. Colden had been living off family capital instead of income since before the Revolution, Fingerhut argues. Therefore, "without the war Colden would have become what he became—a disappointed old man out of step with a new America" (p. 149).

Writing in the tradition of Adrian C. Leiby and Henry J. Young, Fingerhut's intimate knowledge of

local history and sensitivity to apolitical nuances illuminate the way ambivalent loyalism taxed as well as nourished the social cohesion of the new regime.

ROBERT M. CALHOON
University of North Carolina,
Greensboro

PETER CHARLES HOFFER. *Revolution and Regeneration: Life Cycle and the Historical Vision of the Generation of 1776*. Athens: University of Georgia Press. 1983. Pp. xii, 166. Cloth \$15.00, paper \$8.00.

Attempting a "marriage" of the history of ideas and psychohistory, *Revolution and Regeneration* proceeds from the assumption that "our ideas are influenced by our stage in life" (p. 2). Peter Charles Hoffer chooses the younger Revolutionaries of 1776 as a self-conscious "generation" with "a certain outlook, a view of the world," meaning a unique "collective identity" (p. 5). Despite the absence of George Washington, the selection of twenty-one political, legal, and literary leaders of the period, 1776–1815, is broad and reasonable.

Hoffer borrows Erik Erikson's last four stages of life (identity, intimacy, generativity, and ego integrity) and claims that his leadership group struggled through this sequence of personal issues. Rather than "a model we fabricate and then impose upon the past," this series of stages of personality development is, for Hoffer, "a reality we now discern" (p. 5) in the lives of the Revolutionary generation.

The "uses of history" is the subject of *Revolution and Regeneration*. In four substantive chapters it traces conjunctions among the personal development of various members of the group, their roles in the major public events of the day, and their changing understandings of the place of America in the pattern of world history (identity, 1763–76; intimacy, 1776–89; generativity, 1789–1815; and ego integrity, 1800–40). Conscious behavior dominates, in keeping with Hoffer's previous essays stressing academic psychology (cognitive and trait psychology) versus psychoanalytic approaches. Hoffer writes behaviorist psychohistory in nearly pure form: *Revolution and Regeneration* is silent about any important residues of childhood and early youth experiences in adult life.

Hoffer sets his own test of success for this approach: whether "the reader feels a little more enlightened about the deepest meanings of the historical ideas of this generation" (p. xii). By this standard, *Revolution and Regeneration* is only marginally successful. For many of Hoffer's most important points, too little convincing (and sometimes even appropriate) evidence is discussed or cited. Several secondary sources relevant to historical understanding of the period are absent. For instance, the

subject of the role of universal history in its connections with sacred Christian history is discussed only in passing, despite the important work of Perry Miller, Nathan O. Hatch, Philip G. Davidson, and others.

As a marriage of psychohistory and intellectual history, *Revolution and Regeneration* is thoughtful and intriguing for its focus on the actual uses of history by the Revolutionary generation. That important links existed between the personal development, public participation, and changing understanding of America in universal history, ca. 1776–1825, is successfully demonstrated. But the evidence suggests that the problem is more complicated than Hoffer allows; and key issues raised in previous scholarship are never adequately confronted. Finally, this book contends that "for the revolutionary generation, the past was nothing less than the history of the Revolution" (p. 97). Unfortunately the book's limited scope also leads to the inference that historical understanding and the uses of the past by the generation of 1776 amounted to little more than changing evaluations of the Revolution in light of subsequent secular events and personality needs. This is an important finding but a very partial one. Continuity of basic intellectual perspective is lacking. For Hoffer, ideas are determined and not just influenced by stages of life.

WILLIAM J. GILMORE
Stockton State College

JOSEPHINE F. PACHECO, editor. *The Legacy of George Mason*. (George Mason Lectures.) Fairfax, Va.: George Mason University Press or Associated University Presses, Cranbury, N.J. Pp. 149. \$12.00.

This is the most recent book recognizing George Mason as an outstanding leader in the American Revolution and in the development of human rights. It is the first of a projected series, "The George Mason Lectures."

The editor, Josephine F. Pacheco, and the four essayists devote many pages to Mason's career and political thought and to the backgrounds of American Revolutionary thought on human rights. It is refreshing to read that the author of the celebrated Virginia Declaration of Rights greatly influenced many states' bills of rights, was a major figure in the Constitutional Convention, and was "in a sense the motive [force] behind" the Bill of Rights (p. 20).

In the first essay, Ralph Ketcham writes about the never-ending debate on how to protect basic rights from being violated by a tyrannical majority. One side contends that this may best be obtained by a written bill of rights that no governmental agency may contravene. The other side believes that protection lies only in the "values, wisdom and vigilance"

of a wise and educated citizenry (p. 52). After citing the views of such men as Mason, Madison, Jefferson, Hamilton, and justices Jackson and Frankfurter, Ketcham concludes that both forms of protection are needed. Mason would have approved of this conclusion.

Thomas Morris concentrates on the work of the Burger court and of recent activist state courts. Noting the Burger court's tendency to "limit the scope of the Fourth and Fifth Amendments" (p. 68), he shows that a few state courts have been much more activist than the Supreme Court. Even the Virginia Supreme Court has shown some signs of breathing "new life into George Mason's words" (p. 90). The fact that most state judges are elected for limited terms helps to explain their relative lack of activism.

A. E. Dick Howard, known for his writings on the Virginia, English, and American constitutions, shows how the Supreme Court changed its outlook from one that advocated *laissez faire* to one that discovered rights not even listed in the Bill of Rights but rather "'emanations from [its] penumbra'" (p. 107). His appraisal of the Warren court and his comments on the dialectics of constitutional development are instructive.

After giving George Mason credit for influencing the French Declaration of the Rights of Man and of the Citizen and, by implication, the adoption of "written constitutions and bills of rights" in the New World (p. 116), William Barnds discusses political freedom and human rights in Asia and shows how the American desire to help advance the cause of freedom in the world is limited by the necessity of promoting national self-interest and by the knowledge that even a great power cannot impose its wishes on other countries.

The editor and essayists have succeeded in their basic purpose and have also supplemented the works of other scholars. Future lecturers may well explore Mason's economic interests, his views on separation of powers, southern economic interests, state sovereignty and slavery, and his role in the conventions of 1787 and 1788.

George Mason University Press is to be commended for printing an attractive book that is remarkably free of typographical errors and that has a good index.

KENNETH E. ST. CLAIR
Tarkio College

WILLIAM G. MCLOUGHLIN. *Cherokees and Missionaries, 1789-1839*. New Haven: Yale University Press. 1984. Pp. xiii, 375.

Nineteenth-century Cherokee hospitality to Christian missionaries is notorious; yet William G.

McLoughlin is the first to give equal time to all the major missions: Moravian, Presbyterian, Presbyterian/Congregationalist, Baptist, and Methodist. McLoughlin makes unprecedentedly thorough and extensive use of Baptist and Moravian archives, and draws on both Anthony F. C. Wallace's concept of "revitalization" and Robert F. Berkhofer's notion of "factionalism" to interpret his findings. The result is a model study of missionary effort and its impact, bold in interpretation and fascinating in detail.

Following Wallace, McLoughlin adopts an essentially functionalist view of Cherokee Christianity. Military defeat, loss of territory, and depletion of game resources left the Cherokee of 1789 demoralized and impoverished. With some aid from U.S. agents they revitalized their economy through stock-raising, plow agriculture, and the employment of slaves. Plantation agriculture and associated enterprises were accompanied by growing inequality of wealth, the abandonment of communal settlement patterns, and movement in the direction of patriarchal family government. Christian missionaries offered a "progressive" world view, and an individualistic, bourgeois ethic more in keeping with the emerging social order than the static world view and communal ethic of the Cherokee's birthright religion. Missionaries sought to incorporate the Cherokee into a larger Christian community; Baptists and Methodists offered new leadership roles for promising young orators.

Moravian and Congregationalist/Presbyterians of the American Board of Commissioners for Foreign Missions maintained elitist practices that appealed to a rather narrow circle of the mainly mixed-blood aristocracy. Methodists and Baptists, more democratic, made more converts. Methodism also attracted Chief John Ross and several of his relatives. They had no fixed mission stations to arouse suspicion of their designs on Cherokee territory; and their egalitarian behavior made theirs a popular persuasion for the aspiring politician.

During the 1829-39 crisis resulting from the federal government's ultimately successful attempt to remove the tribe from Appalachia, missionaries of all persuasions sympathized with the Cherokee. The American Board proved most active on their behalf on the national scene, but in 1833 they became convinced that removal, like it or not, was best for the tribe. Baptist missionary Evan Jones, who followed Cherokee wishes, remained politically active in the later thirties, and increased his congregation by 500 percent. In these years, Christianity appealed less as a religion of the aspiring than as a creed of the dispossessed.

Christian schools and churches, and the political modernization that facilitated the revitalization of the Cherokee provoked struggles between progressives and traditionalists. McLoughlin accords these

struggles close and detailed attention. He acknowledges that no simple dichotomies (such as full-blood/mixed-blood) characterized the rebellions, and his analysis leaves room for doubt as to whether stable "factions" based on conflict over acculturation developed at all.

During this half-century of struggle over Cherokee acculturation, American racial attitudes developed in a direction that doomed the missionaries' assimilationist goals, while the Cherokee determined to pursue "civilization" as a means to national separatism. Although McLoughlin pursues the theme of missionary failure, he notes that missionaries succeeded better than they knew, as native preachers interpreted Christian ritual and doctrine to accord with the customs and needs of the Cherokee. Frequently he points out how the missionaries' ethnocentrism and condescension limited their reach. Yet Baptists and Methodists were less condescending than McLoughlin would allow, especially toward their native assistants. They offered these brethren respect and autonomy sufficient to enable them to convert Christianity to meet the needs of all classes of the Cherokee.

MARY YOUNG
University of Rochester

RONALD L. HATZENBUEHLER and ROBERT L. IVIE.
Congress Declares War: Rhetoric, Leadership, and Partisanship in the Early Republic. Kent, Ohio: Kent State University Press. 1983. Pp. xiv, 170. \$19.50.

The classic argument about the causes of the War of 1812 revolves around three questions: Who was primarily responsible for leading Congress toward the declaration? Which congressmen supported the decision? And what does this suggest about their fundamental motives? Recently, some broad agreements have tended to emerge. War was an administration measure that divided Congress essentially on party lines. Most Republicans accepted war because they came to think that no other course could secure the republic against internal and external threats to its survival, a conclusion that does not make sense unless we take into account the character of party thought as well as the objective situation of individual congressmen, the president, and the country.

For all its claims to substitute a dynamic "model" of decision making for existing, static explanations of the coming of the war, this new book does not significantly alter recent answers to the basic questions. Although its findings are presented as a challenge to much recent work, a move "beyond the republican synthesis" (chap. 1), they seem in fact most useful for the way they fill existing gaps and reinforce current understandings.

Patently considered, the contribution is quite

real. The authors—a professor of history and a professor of speech—open by objecting that prevailing views do not explain why war was declared in 1812, but not before. In order to account for its timing, they shift the focus of inquiry from the sources of the war to how it was declared. An intensive, quantitative study of the war session, supported by two dozen tables and figures, emphasizes that three "factors" combined to bring the Congress to decision: party loyalty; cooperative, group-oriented leadership from the administration and Republican members of key House committees; and articulation of a rationale for war that had a genuinely national appeal. The model of decision making that results from this examination of the war session is then "tested" against the crises of 1798 and 1807, when one or two of the interlocking factors were absent and war did not result. Finally—and perhaps most interestingly—the book concludes with a novel exploration of the emergence between 1808 and 1812 of a compelling Republican argument for the necessity of force.

Congress Declares War is tough and tedious reading. The early chapters in particular are tortuously organized. The authors carry on a needless quarrel with a "republican synthesis" that is confusingly defined if fully understood. The arguments of secondary works are often poorly summarized, and the comparison of the first session of the Twelfth Congress with earlier crisis sessions rests on assumptions that can certainly be questioned. The work is filled with formidable thickets of dreadful social-science. Nevertheless, it advances understanding both of the leadership and of the rhetorical developments that brought Republicans together in the decision for war.

LANCE BANNING
University of Kentucky

HENDRIK BOORAEM V. *The Formation of the Republican Party in New York: Politics and Conscience in the Antebellum North*. New York: New York University Press. 1983. Pp. 296. \$39.50.

Hendrik Booraem V's stated intentions are modest: not to rewrite the story of the political realignment of the 1850s in New York but to "reorchestrate" it, using different sources and more contemporary historical themes. Following Eric McKittrick's injunction to look at political behavior in the context of the institutional needs of parties, Booraem views the emergence of the Republican party between 1854 and 1856 as it was shaped by local and state politicians, each concerned with using nominations and patronage to build an ideologically viable party capable of winning elections. Making heavy use of local newspapers and the correspondence between

state and local leaders regarding pragmatic party concerns, Booraem reveals the "complex and interwoven relationships that made up the politics of the time" (p. 5). Practical political needs, Booraem asserts, combined with the moral concern over the issues of free soil, temperance, and immigration (the "conscience" of his title) to shape the movement of politicians (and presumably ordinary voters) into the Republican ranks.

Booraem is at his best in exploring the labyrinths of the political power structure in New York. He explains the often overlooked but vital functions, responsibilities, and customary behavior of various party entities: conventions, state and local committees, caucuses, and party newspapers. For the Whig party (but not the Democrats) he identifies those most influential in shaping legislative actions, determining nominations for office, awarding patronage, and influencing the discussion of issues. He brings out the decentralized nature of party organizations, the lack of a strong disciplinary mechanism, and the consequent importance of party identification and loyalty to maintain organizational continuity. Booraem succeeds in showing that institutional party needs combined with concern over new issues to precipitate the Whig party's collapse and shape the strategy used to form the new Republican party.

Booraem is correct, however, in stressing the limited nature of his work. Despite the ongoing debate on the relative importance of issues such as free labor, temperance, and nativism in forming the Republican party, Booraem does little to advance the discussion. He makes some comparison between his work and that of Ronald P. Formisano on political realignment in Michigan, but he otherwise ignores contemporary scholarship. Perhaps because Booraem views voters as being manipulated by party leaders rather than helping to shape the actions of those leaders, he consistently de-emphasizes voting in his study. Although he examines election outcomes in a handful of counties statewide, his analysis is unsystematic. Not only does he fail to explain his criteria for selecting these counties, but he attributes outcomes to factors like the weather, poor organization, or geographic isolation without testing these explanations (along with more traditional socioeconomic and ethnocultural variables) in each county. The timing and character of the migration of former Democratic voters into the Republican ranks is left particularly unclear. Booraem's indifference to the grass roots also leads him to end his analysis prematurely in 1856 at the first hint that the Republican state organization was maturing, rather than when the party's electoral support had solidified and voter turnout returned to normal.

As a study of the inner workings of party machinery in the mid-nineteenth century and a reminder

that party needs are a factor in determining the outcomes of political upheavals, Booraem's study is unique and valuable. This approach, however, is not sufficient to explain the complexities of the political realignment that took place in New York in the 1850s.

PHYLLIS F. FIELD
Ohio University

WILLIAM B. MCCASH. *Thomas R. R. Cobb (1823–1862): The Making of a Southern Nationalist*. Macon, Ga.: Mercer University Press. 1983. Pp. 356. \$18.95.

In the mid-1840s Thomas Reade Rootes Cobb (1823–62) was a well-known lawyer in Athens, Georgia. Eased into lucrative appointive offices with help from his more famous brother, Howell Cobb, and his father-in-law, Judge Joseph Henry Lumpkin of the Georgia Supreme Court, Thomas Cobb became prosperous and his influence grew. He worked hard at his calling, indulging a scholarly bent to produce an authoritative digest of Georgia's laws in 1851, a comprehensive codification of the state's statutes in 1861, and—in the interim—his learned and often-quoted defense of slavery, *An Inquiry into the Law of Negro Slavery in the United States* (1858). Cobb campaigned for secession, played a leading role in Georgia's secession convention, served as a delegate to the Confederate convention in Montgomery, raised a legion, and rose to the rank of brigadier general before he died of a leg wound at Fredericksburg.

William B. McCash has written the full biography that Cobb has long deserved. Limited by the relatively sparse documentation for Cobb's first thirty-six years, McCash skillfully synthesizes information from many different sources. Chapters on Cobb's career as a lawyer, his devotion to evangelical Protestantism, and his work as an educational reformer occupy the first half of the book. They cast fresh light on Cobb's character and undercurrents of small-town society revealed by such episodes as the choir strike at the First Presbyterian Church precipitated by the refusal of Cobb and the other trustees to agree to hire a talented organist who happened to be a Jew. McCash emphasizes that Cobb's conservatism set him apart from evangelicals and reformers in the North and led him to defend slavery and southern distinctiveness, then secession and the Confederacy, subjects considered in the last half of the book. The record of Cobb's activity is more complete as he moved increasingly into public life in the late 1850s, but McCash's narration of his career as a secessionist and Confederate reenforces the main features of previous accounts of the final three years of his life, with the exception that McCash insists on the evangelical roots of Cobb's social and political ideas.

Students of antebellum legal and political history will find the book informative and useful. To historians of southern families the book may prove intriguing. Although McCash asserts that Cobb's religious convictions made him "a truly representative southerner" (p. 325), one wonders if his representativeness is as arresting as his uniqueness. McCash makes scattered comments about Cobb's pinched, officious manner and his inclination toward self-pity, but a complete portrait of Cobb's personality never quite comes into focus. In some ways it seems that the central fact of Cobb's life was that he was Howell Cobb's younger brother.

MICHAEL P. JOHNSON
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Irvine

CELIA MORRIS ECKHARDT. *Fanny Wright: Rebel in America*. Cambridge: Harvard University Press. 1984. Pp. xii, 337. \$22.50.

The outline of Frances Wright's life has long been known: the involvement with Lafayette, the travels in the United States, New Harmony and Nashoba, the career as lecturer for radical causes in a recalcitrant United States. Through careful scholarship in sources previously unknown or underutilized, Celia Morris Eckhardt has illumed the interstices of the episodes of Wright's life, providing us with the kind of detailed account we have come to expect from a definitive biographical statement. Moreover, Eckhardt has deepened our understanding of Wright as a person, a thinker, and an activist, revealing her strengths and weaknesses and both placing her squarely in the context of her times and elucidating the larger historical dimension in which she played her part.

Dealing with Wright's life, Eckhardt doubly faces the common conundrum of the biographer: the question of the relationship between individual psychology and social context, of individual blame versus social and parental responsibility. For Wright was extreme about almost everything in her personal life as well as in her political life. She constantly roamed across the American continent and back and forth to Europe. At the age of forty, she seemingly acceded to the conventional sexual views she had attacked all her life by retreating into an unhappy marriage with an inappropriate partner because she did not want the social obloquy of raising an illegitimate daughter. She bonded with a beloved sister but for the most part rejected her own child. She is, as many contemporaries found her, a difficult subject indeed. She had neither Elizabeth Cady Stanton's sense of humor nor Susan B. Anthony's ability to make her public and personal life synonymous. Wright's abstract intellectualism, by

Eckhardt's analysis, allowed her to forge a powerful radical critique but blinded her to social realities. Her egocentrism allowed her to take on masculine roles few other women of her age dared assume but engendered a heroic ambition not only ultimately unattainable but also alienating to others. Powerfully drawn to male role models, she only rarely retreated to that community of women that, recent feminist scholarship tells us, sustained many nineteenth-century women activists. (In fact, her life offers an interesting critique of the ubiquity of the "separate sphere" model of behavior.) Of all nineteenth-century women activists, she most resembles Victoria Woodhull, and it is curious that Eckhardt misses the resemblance.

Was Wright the victim of a traumatic childhood? Did she suffer from a "manic-depressive" syndrome? Was she in reality so out of tune with her times that consistent failure was inevitable? Eckhardt at one point or another advances all of these theses; one wishes that she had more systematically pursued their interactions. I am not an advocate of extreme psychobiography, but in the case of Wright, more in-depth theoretical probing might have clarified a character whose dimensions still remain murky (although I have the impression that at certain key points in Wright's life the documentation simply becomes inadequate, and I wish that Eckhardt had been clearer about this).

Still I applaud Eckhardt's ultimate conclusion: Frances Wright stands as a model for all women who have come after her; her vision was neither fully pursued nor realized until the 1960s. The scope of her understanding of the nature of social ills was enormous; it is to the benefit of all that she existed.

LOIS W. BANNER
University of Southern California

BRIGHAM D. MADSEN. *Gold Rush Sojourners in Great Salt Lake City, 1849 and 1850*. (University of Utah Publications in the American West, number 18.) Salt Lake City: University of Utah Press. 1983. Pp. xii, 178. \$17.50.

Most readers of Utah history are generally aware of the mutually beneficial relationships between the gold rush argonauts and the Mormon settlers in Salt Lake City in 1849 and 1850, but Brigham D. Madsen's excellent study adds a depth and perspective to the story that makes his book "must" reading for anyone interested in either group. It is based solidly on primary sources: gold rush diaries, letters, Mormon journals, and contemporary publications. From such documents Madsen provides his readers with a new feel for early Salt Lake City as experienced by those who stopped for a few days or longer

on their way to California. He skillfully weaves together the details of an engrossing story that, he argues, "changed American history" (p. 132). That statement may be a slight exaggeration, but he demonstrates that the contact between the two groups clearly had an important influence on the course of both Mormon and western history. The City of the Saints was suddenly "established as a mercantile crossroads," leading the national government to use it as a base for further western exploration. Reports of gold seekers as well as government officials brought Mormon society to the attention of the nation at large, and all this "broke the isolation of the Mormon settlers and focused national attention once again on the 'Mormon problem.'"

Even before arriving at Salt Lake City, the argonauts found Mormons along the overland trail, waiting to turn a profit while providing such necessary services as blacksmithing and ferrying. More significant for the Mormons, however, was the windfall that came as many hapless emigrants abandoned all kinds of goods along the trail in order to lighten their loads and ease the strain on their weakened animals. The Mormons were able to scavenge guns, farming equipment, stoves, cooking utensils, harnesses, clothing, groceries, and many other things badly needed in the two-year-old Salt Lake City. When the argonauts reached Salt Lake City they could purchase fresh animals, milk, butter, garden vegetables, and repair work, although usually at highly inflated prices. Those who had not abandoned all their surplus wagons, carriages, harnesses, groceries, tea, coffee, flour, liquor, and other things needed by the Mormons sold them there at sacrifice prices.

In general, the relationship between the Mormons and the overland travelers was very good, although tension was not totally absent and Madsen documents both experiences very well. In addition, the emigrants often quarrelled among themselves, usually over personal property, and when they reached Salt Lake City in 1849 they found themselves taking their disagreements to the only courts available: the Mormon bishop's courts. Madsen also reports on other things the emigrants saw and heard, all of which provides fascinating insight into many social, religious, and political aspects of early Salt Lake City history.

Madsen adds enough additional detail to help the reader understand the western setting in which the story took place. He deals effectively, for instance, with the travel choices the emigrants had to make after their short stay among the Mormons. The political setting, too, is handled well, although Madsen inadvertently perpetuates one long-standing historical error (p. 72). According to tradition, when the Mormons were attempting to obtain statehood for their territory they held a convention in March

1849, which resulted in a constitution for the Provisional State of Deseret. Recently, however, Peter Crawley has demonstrated that the convention never took place. It was a fiction, he concludes, invented because "Congress would not have considered an application that had not been produced by a constituent convention and ratified by popular election" (*Friends of the Brigham Young University Library Newsletter*, 1982). In general, however, *Gold Rush Sojourners* is a fine, accurate, and well-written addition to western Americana.

JAMES B. ALLEN
Brigham Young University

NORMA LOIS PETERSON. *Littleton Waller Tazewell*. Charlottesville: University Press of Virginia. 1983. Pp. xii, 311. \$25.00.

Littleton Waller Tazewell enjoyed an immense reputation in his day, but in our time he is little known. His contemporaries, many of whom agreed with one another on little else, esteemed and respected his intellect and integrity. If he had chosen to give encouragement to those who endorsed him, he might well have served with distinction in the highest offices in the country. But, as John Tyler said of him, Tazewell was "the most unambitious of men," and Edmund Ruffin simply could not understand how such a talented and potentially powerful person could decline the opportunities he had to seize the helm. What a pity, Ruffin wrote, that such a gifted individual "has been of scarcely any use to the public." Presidents courted him; his contemporaries elected him to the House of Representatives, the Senate, the governorship of Virginia, though he never sought public office and disliked its burdens so intensely that he abruptly resigned his Senate seat and the governorship.

One of the reasons for the leanness of his reputation today is that there has never before been a full biography. Now Norma Lois Peterson has provided us with an exemplary one, drawing on the Tazewell papers in the Virginia State Library and the writings of those who were closely associated with Tazewell. The great value of this book is not so much that it presents new interpretations of Virginia and the nation between 1800 and 1860, as that the author has skillfully used the career of Tazewell to flesh out our knowledge and appreciation of many events. Tazewell was at the center of such affairs as the trials of Burr and Barron, the "Old Republican" movement, the controversy over the embargo and the War of 1812. Always independent minded, fearful of majoritarian democracy, a vehement supporter of the right of secession but not of the right of nullification, Tazewell was beholden to no one.

Peterson uses the long career of this rather cold,

haughty, individualistic Virginian to illuminate the politics of the decades from Jefferson through Van Buren. Tazewell lived on to the very eve of the Civil War, but there is a much less thorough coverage of the last quarter century of his life. The author's reasons are understandable—Tazewell was out of public life—but the scantier treatment of the 1850s is a bit disappointing. This is, however, an excellent, valuable biography.

WILLIAM M. DABNEY
University of New Mexico

WILLIAM HANCHETT. *The Lincoln Murder Conspiracies*. Urbana: University of Illinois Press. 1983. Pp. 303. \$18.50.

Don E. Fehrenbacher has noted that the Lincoln assassination is "one of the greatest real-life murder mysteries of all time." With *The Lincoln Murder Conspiracies*, a well-written, carefully researched, and sensibly argued study that complements Thomas Reed Turner's *Beware the People Weeping* (1982), William Hanchett becomes the finest student of that mystery and, more important, of the literature it has engendered.

The book briefly delineates the climate of opinion that spawned Lincoln's assassins, emphasizing that many Americans, North and South, believed that the "tyrant-president" deserved killing. Hanchett shows clearly the circumstances of Lincoln's death, although untied knots, unlike in most detective novels but as in all historical research, remain. At the trial of the conspirators, of all culprits, the U.S. Government produced the first of many unsubstantiated theories about a grand conspiracy behind the deed of John Wilkes Booth. After the government prosecutors' failure to implicate Jefferson Davis and associates, the dominant picture accepted by the nation showed Lincoln's murder to be the work of a small band of misfits. Minor myths continued, about Booth living into old age, for example, or the Vatican being behind the murder. So did the often petty squabbles about the guilt of some of the conspirators, most notably Mrs. Mary Surratt whose execution, and "after life," deserves a solid feminist analysis.

Chemist turned amateur historian, Otto Eisenschiml, the chief villain of Hanchett's work, began a new era of great conspiracy theories in the late 1930s. He ingeniously argued that the diabolical secretary of war, Edwin M. Stanton, was behind the murder. Since then the professional historians' saner voices have been sometimes confused and almost always muffled by a revolving barrier of speculative works promoted by the Book-of-the-Month Club, the *Reader's Digest*, cheap paperbacks, cinema, television, and (if the publisher is to be trusted) a million

copy paperback bestseller *The Lincoln Conspiracy* (1977). The authors of this last work received an honorary degree from a Tennessee college and also the Freedom Foundation's George Washington Medal of Honor. The film made from their book can be seen on late night television and appears to be the most influential current source of information about the Lincoln assassination. The same situation may be true for the rest of an interested world, which has been favored with the translation of sensationalized assassination books into numerous languages.

Superior sleuth Hanchett, professor of History at San Diego State University, writes on an important subject and it is imperative that we, professional historians, who hold up our elitist noses at such fare, understand this. Letting a subject that has captured the public imagination fall into the dubious hands of the unprofessional, the speculator (financial or historical), and the honest paranoid has dire consequences. In the United States it cultivates native paranoia and abroad an antipathetic view of a deeply conspiratorial America. It weakens democracy everywhere.

GABOR S. BORITT
Gettysburg College

HAROLD HOLZER *et al.* *The Lincoln Image: Abraham Lincoln and the Popular Print*. New York: Charles Scribner's Sons. 1984. Pp. xxi, 234. \$35.00.

In this illustration-saturated age it is well to be reminded that mid-nineteenth-century Americans had only the vaguest idea what their heroes looked like. But in the new participatory democracy they were eager to learn, and printmakers responded to the demand. Using photographs and paintings for models and drawing liberally on their imaginations, lithographers and engravers produced great multitudes of single sheet prints of the nation's leaders, which the people displayed in their homes as today they display posters and art reproductions. This book, growing out of an exhibition of prints featuring Abraham Lincoln from the Louis A. Warren Lincoln Library and Museum, Fort Wayne, Indiana, and initially exhibited at Gettysburg College to commemorate the 175th anniversary of Lincoln's birth, presents a representative sample. It consists of 102 print reproductions, the originals from which many were drawn, and an interesting expository text.

The first known Lincoln print, a woodcut from an 1857 photograph, made its appearance at the 1860 Republican nominating convention, when copies were rained on the delegates from the balconies in a remarkably modern demonstration of image building. But the power of prints to shape public atti-

tudes was not fully appreciated, or else, Harold Holzer and his coauthors observe, Republicans would have portrayed the president during the Civil War as he was portrayed extensively after it: as a family man incapable of the tyrannies alleged by the opposition. Although Lincoln had less family life in the White House than before, postassassination printmakers showed him with his wife and sons in loving family circles, thus magnifying grief at his death and perhaps originating the craze for first family pictures and stories.

During much of the war, generals were more popular than politicians (even presidents) as subjects for prints, but the Emancipation Proclamation gave Lincoln special distinction. Ordinary Americans, studying Alexander Hay Ritchie's superb engraving of Francis B. Carpenter's painting of Lincoln holding the proclamation in the presence of his cabinet, could share a great moment in history. No matter that most printmakers were motivated by commercial considerations. Their portrayals of Lincoln, whether favorable or not, showed him in relation to the events of his time in ways that photographs and formal paintings could not, thus creating a new sense of immediacy and popular involvement. Their representations of Honest Abe, of the Great Emancipator, and of the nation's beloved martyr wrenched from his family to join George Washington in heaven, helped to shape the enduring Lincoln image.

If the prints lack the excitement of others popular at the time—the gold rush, clipper ships, great disasters—it is no doubt because an individual cannot compete with such dramatic scenes. But being published here exclusively in black and white is also responsible; in color many of the Lincoln prints take on unexpected vitality. Nevertheless, *The Lincoln Image* shows Lincoln as contemporary Americans knew him and is important social history.

WILLIAM HANCHETT
San Diego State University

VICTOR B. HOWARD. *Black Liberation in Kentucky: Emancipation and Freedom, 1862–1884*. Lexington: University Press of Kentucky. 1983. Pp. vii, 222. \$23.00.

JULIET E. K. WALKER. *Free Frank: A Black Pioneer on the Antebellum Frontier*. Lexington: University Press of Kentucky. 1983. Pp. xii, 223. \$20.00.

In a splendid historical biography, Juliet E. K. Walker chronicles the life of her great-great-grandfather, Free Frank, a slave of mixed ancestry born in the South Carolina upcountry frontier in 1777. Frank skillfully manipulated the "competitive market for hired slaves on the frontier" and bought

freedom for himself and his family. The ease with which slaves could hire out their labor and the absence of a threatening imbalance in the black-white population ratio gave blacks on the frontier a better lease on economic opportunity than in other regions of the South. Before his death in 1840, Frank's odyssey from slavery to freedom was played out across a succession of western frontiers from the Kentucky Pennyroyal to Illinois in the Old Northwest.

Walker presents a vivid description of the rigors of life for slaves and whites as well as the dynamics of race relations on the frontier. Her discussions of race in the history of the frontier, of the conflict-consensus model of community development, and of the frontier as a germinal point for the American character are, however, somewhat diffuse and ill digested.

Given the collective weight of racial animosity and the hardships that were the accustomed lot of most pioneers, it is remarkable that Frank was able to establish stable tenancy on the land. That he showed an economic profit in the bargain makes his achievement all the more significant. Frank ran a gauntlet of racial proscriptions in federal and state law to found a town, New Philadelphia, Illinois. In view of the checkered career of most towns that mercurially appeared and disappeared on the frontier, New Philadelphia was a bounding success. As the locus of institutions such as the post office and as a nexus of transportation, Frank's town competed favorably with other towns for preeminence in the county.

New Philadelphia was a station on the underground railroad and the site of considerable activity by the National Negro Convention. That forum, which developed into a major community institution, put New Philadelphia in touch with the affairs of the nation and apprised its citizens of the evolving agenda for race progress.

This narrative of a slave who overcame racial barriers, previous condition of servitude, and the rude shocks of frontier settlement makes captivating reading. Walker variously describes Frank in terms of his "eagerness, industry, and reliability." He was otherwise noted for "ingenuity, resourcefulness, energy, [and] formidable business acumen." Walker's crowning generalization is that Frank was "a man of uncommon drive and determination [who] would have had historical significance even if he had not been a slave." Therein lies the strength and the paradox of this biography. However illuminating as the account of an exceptional man, it is difficult to identify Frank's remarkable life within a larger catalogue of antebellum Afro-American experience. I heartily concur with Walker's tacit premise that there is no generic Negro and that black life was not merely a case study in pathology. Still, the lack of typicality is definitive. Frank's achievement was

more than a black experience within an American design; it was the embodiment of the ideology and values emerging under the rubric of Jacksonian Democracy and Manifest Destiny. Be that as it may, *Free Frank* achieves the "poetry" without the "obtrusive egotism" that R. G. Collingwood finds typical of historical biography.

Victor B. Howard's *Black Liberation in Kentucky* is a good state history, but it belies its title somewhat in the first half of the book. Throughout that discussion blacks are merely pawns in the sectional crisis, although Howard skillfully uses his sources to explore the development of black laws and race relations. His analysis is largely devoted to the minutiae of the case for and against unionism and the evolution of Lincoln's war aims with the teetering imbalances of border politics in Kentucky as "a turning point in the scale." Despite the particular significance of Kentucky, Howard's account of race as an ingredient in public policy and in private attitudes correlates with the findings of other monographs on border states, thereby giving his discussion the aura of a twice-told tale.

From relative obscurity as a direct reference point for the black experience, the book springs alive at midpoint with a fine analysis of the black family as a mooring for identity and as a building block of community. Howard offers a poignant and insightful description of black soldiers for whom "the depth of kinship" and the integrity of the family were of unqualified importance even as they were hounded by the bigotry of both northern and southern whites. The analysis of family would have been even more informative if Howard had generated statistics regarding work and wage patterns as well as gender, age, and head of household from the *Record of Contracts with Freedmen*, the source for much of his information.

Howard describes the "coordinated black movement" to secure equal rights, and he is on sound ground in his summary statement: "If blacks themselves had not taken the initiative to abandon slavery there would have been no emancipation in 1863." Suffrage campaigns and organized labor were among the several channels for the exigent demand for freedom. Similar Kentucky organizations provided hotbeds of Kansas Fever, which culminated in the Great Exodus of 1879.

Howard's analysis of black activism in Lexington is of one piece with Walker's discussion of the Negro Convention Movement in New Philadelphia. Both historians demonstrate that organized protest was an important pillar of the community and an emblem of black intellectual culture. The durability and the intensity of the desire for freedom was a constant symbol of the aspirations of the black Kentucky soldier and of Free Frank. Howard and Walker identify significant units of social structure

at a successively wider optic, from the perception of self, to the family, and to organized protest. These important monographs contribute substantially to the growing body of scholarship that reveals a black community of considerable vitality and stability at the dawn of freedom.

THOMAS C. COX

University of Southern California

RANDOLPH B. CAMPBELL. *A Southern Community in Crisis: Harrison County, Texas, 1850-1880*. Austin: Texas State Historical Association. 1983. Pp. xviii, 443.

A Southern Community in Crisis is an account of slavery, secession, war, and Reconstruction in Harrison County, Texas, based on traditional sources (documents and family papers) as well as four samples from each of the manuscript censuses, 1850-80. Harrison was a black-majority, cotton-growing county on the eastern border of the state, legitimately part of the antebellum plantation belt. Hence, the book invites comparison with other recent plantation area studies, and most of its topics are drawn from that literature: wealth distribution and the profitability of slavery before the war, the rise of sharecropping and the politics of railroads, schools and redemption after the war. The research is competent and remarkably thorough, but the book lacks sharply defined themes and has a tendency to blandness. For example, the chapter on slavery concludes: "For whites, then slavery was in many ways a valuable but troublesome institution" (p. 145); and the last sentence in the book states: "Continuity matched change in Harrison County from 1850 to 1880; the Old South had not and would not change its ways easily" (p. 395).

Although Randolph B. Campbell does not write with the provocativeness of Jonathan Wiener nor the insight of Michael Wayne, he has some good material and the book is well worth reading. He devotes a moving chapter to the wartime letters of a young Harrison County husband and wife. Particularly interesting is the husband's accurate perception that landed property would be much more secure after the war than money or slaves (pp. 237, 393). (But how many others saw this and at what point during the war?) The detailed treatment of postwar politics is also informative. Campbell shows that an 1872 petition for a railroad bond issue was signed by men of all political views, even though the resulting indebtedness was later used to attack the Republicans. Because of its black majority, Harrison County was late being "redeemed," but between 1878 and 1880 the Republicans were ousted here too, and Campbell's account raises perplexing questions. Although the redeemers used many tactics, it is clear

that substantial numbers of blacks voted for them, and overt signs of intimidation and fraud are scarce (chap. 13). Campbell asserts that the "mass of Negroes . . . were through no fault of their own, unprepared for politics" (p. 364), but one does not come away with the feeling that these events have been fully explained.

The general problem with the book is that the author has tried to cover an entire array of social, political, and economic topics, but he has not thought carefully about which of these are appropriately pursued at the county level, and which are not. Gini coefficients and even "social persistence rates" for an individual county are only marginally informative, whereas case studies of local political dynamics and plantation management are much needed. This particular local study is not all one might hope for, but it is certainly a worthwhile addition to the literature on the transition from slavery to freedom.

GAVIN WRIGHT
Stanford University

ROBERT STEVENS. *Law School: Legal Education in America from the 1850s to the 1980s*. (Studies in Legal History.) Chapel Hill: University of North Carolina Press. 1983. Pp. xvi, 334. \$19.95.

With clarity, style, and wit Robert Stevens—formerly of the Yale law faculty, and now president of Haverford College—tells of one-hundred-thirty years in American legal education. For lawyers this account authoritatively charts relations between law schools and the bar, surpassing any previous treatment. But readers of this review should not pass it by as relevant only to the lawyers.

Law has been so implicated in the country's life as to make it a proper concern to scholars in the social disciplines. Many able undergraduates head into law schools. Most leading universities today include a law school and must relate those schools to university missions. On all these counts Stevens tells much that is relevant to social scientists; at the least they should read chapter 8 (on efforts to relate law studies to social science knowledge) and chapter 13 (on gathering tensions of values between law faculties and law students and within the faculties).

Two themes will particularly concern nonlawyers. Social reality includes existence of differences among socioeconomic classes, and among needs and demands of individuals and of organizations, and of individuals in different functional roles. Differences follow in the work that lawyers do for various types of clients, most sharply between the work of lawyers serving business corporations or private wealth and of lawyers serving individuals of ordinary or modest means. But legal education has tended to ignore

these realities, defining goals and standards of law schools as if they served a homogeneous bar and population, and should be judged by a uniform measure. Stevens is persuasive in pointing to basic disjunctions between social reality and the general approach of legal education.

The university rates advancing knowledge as a prime responsibility. To hold title in the university, a law school should also commit itself to scholarly inquiry into the nature and quality of legal order. But Stevens finds persistent tension between the scholarly mission and pressure from the bar and from legal educators' professional traditions to assign primacy to training students to be client caretakers. The tension is perhaps to be lived with, rather than resolved. In the background, also, is chronic underfunding. Over one hundred years' predominance of case method instruction has taught those who hold purse strings to expect legal education to be cheap, marked by large classes, insufficient faculty, and scant free time for research. Underfunding also cramps improved preparation for law practice; good clinical instruction is expensive. These limitations breed low morale among law students, past the first year of a three-year course.

Stevens accurately estimates legal education in its present position. That position I would rate, however, as an encouraging advance in depth and range over what I encountered as a law student in the early 1930s. Were there even modest increments in support, the record of our best thirty or forty law schools indicates a promising creative potential, bearing both on scholarship and on training for practice.

WILLARD HURST
Law School,
University of Wisconsin

ELIZABETH JOHNS. *Thomas Eakins: The Heroism of Modern Life*. Princeton: Princeton University Press. 1983. Pp. xx, 207. \$42.50.

The post-Appomattox decade of the 1870s unfolded in the United States to the accompaniment of the scandals of the second Grant administration. The political peccadillos in Washington were fueled in the industrial arena—as the Massachusetts Brahmin Charles Francis Adams, Jr. testified in the *North American Review* in 1871—"by remarkable examples of organized lawlessness under the form of law" especially on the part of railroad barons bent on reducing "courts, legislatures, and sovereign States to an unqualified obedience to their will."

In brief, the golden age of America that genteel Eastern publicists believed they discerned in the generation before the Civil War was now over, its place taken by an acquisitive nouveau-riche society

and aptly dubbed the Gilded Age. American heroism, it seemed to Adams and his cultured New England friends, had ended.

Not so in Philadelphia, argues art historian Elizabeth Johns. To the City of Brotherly Love, she recounts, Thomas Eakins eagerly returned in 1870, fresh from his art studies in France. Unlike his similarly talented fellow Philadelphian Mary Cassatt, who elected to forego a life of wealth to remain in France, Eakins took deep pride in his home town and he was restless to begin celebrating its leaders and their heroic achievements on canvas.

If Eakins was sensitive to any antisocial goings on during the 1870s and 1880s, the author is ignorant of this. She does not mention the Gilded Age. Eakin's intention as a portrait painter, she states, was to interpret the "heroic, disciplined, and democratic possibility [sic] that were dominant beliefs of the age" (dust jacket) by celebrating the "ideal of the moral, disciplined, self-made man" (pp. 168–69). Although the artist did ultimately come to his senses, Johns is a bit carried away in touting his intellect, even suggesting that the later nineteenth century might appropriately be named "The Age of Eakins" (p. 168).

Yet there is much to praise in this book. A slim volume of 169 pages of text, intended for the specialist, it makes no attempt to be a biography. Instead it presents five distinct essays dealing in documented fashion with portrait subjects of Eakins, including an interesting concluding essay on Walt Whitman. Taking for granted that Eakins is the foremost American painter of the nineteenth century, the author seeks to show how he "made the portrait the vehicle for his study of man" (p. 168). But—according to Johns—although from 1870 to 1913 Eakins successfully depicted the "heroism of modern life," in the process he alienated nearly everyone. The principal reasons were his intransigence and his insistence on portraying sitters with all their warts and as older than they were.

Still, argues Johns, the stereotype of Eakins as an uncompromising realist needs correction; for in nearly all his paintings "there are large areas in which his technique is free, experimental, and sometimes even deliberately clumsy" (p. xx).

Attentively edited—the book is unblemished by typesetters' errors—it is an altogether attractively designed volume. The photographs, of which there are 16 color plates and 124 black and white, usefully illustrate the author's points. There is a good bibliographic essay.

WILLIAM M. ARMSTRONG
Potsdam, N.Y.

AMOS J. LOVEDAY, JR. *The Rise and Decline of the American Cut Nail Industry: A Study of the Interrelation-*

ships of Technology, Business Organization, and Management Techniques. (Contributions in Economics and History, number 53.) Westport, Conn.: Greenwood, 1983. Pp. xx, 160.

This is an important subject improperly presented. First, it is far too brief. The text is divided into five chapters that vary in length from nine to thirty pages; together with an introduction of seven pages and a conclusion of four, the entire text totals a mere one-hundred-twenty-seven pages. Second, the conceptualization of the subject reflects this short framework. To Amos J. Loveday's credit, the chapters are arranged logically and track one to the other. But the perspective on the subject never rises above it; it remains confined to the internal evolution of the industry. Occasionally, the author uses another historian's idea about more general changes in technology, market structure, and management, but they intrude into the narrative. Loveday knows the evolution of the cut nail industry but either does not understand the larger context of American business and economic history of the nineteenth century or is unappreciative of its role surrounding the development of that industry. In either case, the two appear incompatible in this volume. Third and last is the author's tortured prose. George Orwell once said that grammar is of no importance as long as we make our meaning plain. Well, Loveday's meaning is frequently not clear. Leaving aside poor syntax, whole paragraphs are murky at best and their first sentences rarely function as topic sentences. A good copy editor with an understanding of American business and economic history should have read this manuscript for the press.

Now for the good news. Loveday is to be commended for giving us our only study of this important industry, and he makes several contributions to what we know about it. First, the mass production of cut nails (for readers unfamiliar with what cut nails were: they were iron versions of today's flattened concrete and stone nails) by machines had already occurred by the time of the Civil War; the center of the industry had located in and around Wheeling, West Virginia; and nail makers reduced costs during the deflationary generation following the Civil War by constructing blast furnaces that internalized the production of pig metal and by developing modern accounting and professional management techniques that expanded the decision-making process. Wheeling became America's "nail city" from 1860 to 1880, the Amalgamated Association of Iron and Steel Workers was formed in 1875, and the Western Nail Association was founded during America's centennial year.

But then came steel, labor problems during the 1880s, and the wire or round nail that would replace the cut nail within one generation. Loveday's story

ends about the time of World War I. Wire nails were cheaper and easier to use, and wire nail makers combined with barbed wire manufacturers during the 1890s to give greater strength and internal cohesion to that industry than cut nail makers could hope for. The effect on Wheeling was devastating but mitigated somewhat by the McKinley tariff that allowed those mills that would and could to turn to the manufacture of tinplate.

On balance, this is a micro-business history that covers its subject adequately rather than exhaustively while leaving several other areas open for further research.

GARY L. BROWNE
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DAVID O. WHITTEN. *The Emergence of Giant Enterprise, 1860–1914*. (Contributions in Economics and Economic History, number 54.) Westport, Conn.: Greenwood. 1983. Pp. xii, 196. \$29.95.

I am at a loss to determine a possible audience for this book. It is not sufficiently comprehensive to qualify as a text in business history (no discussion of the steel industry and only a mention of oil, for example). Written from secondary sources, and only a handful of them at that, it is neither a scholarly monograph nor a commentary on work in the field. As the author acknowledges, it offers no original interpretative framework, such as John Galbraith's concept of countervailing power.

Most of the book is taken up with a series of case studies focusing on the question of structure. Some chapters deal with whole industries (mining, forest products, communications and transportation), and others take up individual companies (United Fruit, Singer, American Sugar Refining, American Tobacco). The most fully documented and freshest account summarizes the rise and decline of the giant farms of the late nineteenth century. The chapter on forest products, constructed from three books and an article, is hopelessly inadequate even as a capsule description of major developments.

One could overlook the research flaws if the author had a new and interesting theory to unite these case studies; Galbraith, after all, gets away with hardly a footnote. But David O. Whitten seems less interested in testing ideas about the rise of big business than in convincing the reader that the big businesses of the day—even the outright monopolies—got that way because of economies of scale, the imperatives of technology, and the need to cover fixed costs. "Giant firms," as he puts it, "were the product of American economic conditions, not avarice and greed" (p. 149).

There is much truth in this, the economists' view;

and certainly this reviewer has too often decried the perils of competition in the coal mines and sweatshops to now advocate an economic system grounded in small units of production. But in his praise of bigness and in his mechanistic analysis of its causes, Whitten is injudicious. Did Standard Oil—to raise a case ignored here—have to control 90 percent of the nation's refining capacity in order to stabilize markets and be truly efficient? Was American Sugar Refining just big enough to achieve full economies of scale, and no bigger? Were consumers—a group hardly mentioned in this study—well served by conglomerates such as American Tobacco? No, no, and no.

WILLIAM GRAEBNER
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Fredonia

WALTER GALENSON. *The United Brotherhood of Carpenters: The First Hundred Years*. (Wertheim Publications in Industrial Relations.) Cambridge: Harvard University Press. 1983. Pp. vii, 440. \$25.00.

Walter Galenson has written the first history of the carpenters' union since Robert Christie's *Empire in Wood* in 1956. The two volumes differ in their view of the union. Christie was critical of the direction taken by the carpenters following the departure of P. J. McGuire in 1901. Galenson generally supports the policies of the union. He argues that the carpenters fully represented American-style business unionism. This type of activity succeeded because it met the needs of the members despite a hostile environment. Unlike Europe, American unions could not build on a homogeneous, class-conscious, blue-collar workforce, nor take advantage of a more fluid political party system. In addition, the high rate of American economic growth allowed for substantial and continuing improvement in the wages and working conditions of union members through the techniques of business unionism.

For the carpenters, these methods included the constant expansion of jurisdiction to increase bargaining power; the related concern with the organization of mill workers to allow the use of the union label; the acceptance of less competition and higher prices for construction if this were necessary to provide greater benefits for the members; the focus on economic gains for the members as the *raison d'être*, and a consequent refusal to view the union as an instrument of broader change, or even as a force for solidarity within the labor movement; the reliance, until 1964, on a nonpartisan political policy; and, until the post-World War II period, an acceptance of voluntarism, with its rejection of the welfare state and the interference of government in labor relations. These themes are not new, but Galenson

skillfully shows how they developed from the conditions in the trade, and how they changed over time.

Galenson's book will not please those who regard the carpenters' union as a leading member of the most hindbound group in the American labor movement. It will, however, appeal to readers who want to confront the reality of the dominant sector of organized labor prior to the changes of the 1930s. This is a book for those who are not content with what might have been or what should have been. Galenson's book clearly delineates what the carpenters did, and why they did it.

The weakest element in the book flows from the author's decision to cross the line between sympathetic explanation and justification. Discussion of why the union acted as it did, even from the perspective of its leaders, would have been sufficient; defense, and occasional criticism, of such actions was unnecessary.

This volume draws on the long tradition in American labor history that reaches back to John Commons, and includes Selig Perlman and Philip Taft. As historians properly pursue the now not so new "new" labor history, with its emphasis on the worker rather than the trade union, this book shows that there is still much of value to be written about the labor movement. After all, organized labor has provided the only effective access to economic influence, and, more recently, political power for workers in America.

IRWIN YELLOWITZ
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City University of New York

SIDNEY L. HARRING. *Policing a Class Society: The Experience of American Cities, 1865-1915.* (Crime, Law, and Deviance.) New Brunswick, N.J.: Rutgers University Press. 1983. Pp. xii, 301. \$30.00.

Readers' responses to this book will probably depend in large measure on their level of agreement or disagreement with its theoretical and ideological base. Sidney L. Haring begins with a chapter outlining his version of the Marxist theory of the capitalist state in general and municipal police forces in particular. To a more skeptical reader, these theoretical statements become previously prepared positions to be defended throughout the remainder of the text rather than hypotheses to be tested against the available evidence. To Haring, there is no room or reason for ambiguity or uncertainty. In a capitalist state, government exists for the promotion and protection of the reproduction and accumulation of capital; and the police function is the violent repression of the working class so that this reproduction and accumulation can proceed unimpeded. Even if strikes and antistrike activity have become less violent over time or police have spent

most of their active duty time on service provision, these developments merely serve to legitimate continuing class divisions and class conflict and provide justification for police intervention in workers' lives. In the same way, any evidence that people were more concerned about ethnicity or religion than class can be explained as something more apparent than real, or an indication of false consciousness.

The supporting examples for this interpretation come from Great Lakes industrial cities, especially Buffalo, Milwaukee, and Chicago, between 1865 and 1915. All were growing rapidly with police departments growing even more rapidly than total population. Haring disputes the traditional picture that late-nineteenth-century police agencies were mismanaged, dominated by political hacks, and inefficient. Rather they were formidable organizations for helping employers break strikes, and even though they could not or would not satisfy all upper- and middle-class demands for controlling workers both on and off the job, their influence on workers' behavior must be recognized.

These arguments have some force, but they are presented in an absolutist fashion that admits of no exceptions, conflicting evidence, or alternative explanations. The author says nothing about gender and age ratios and differences within immigrant, working-class areas, nor does he inquire into differences between working-class men and women on such issues as gambling, prostitution, and excessive drinking or police activity or lack of it. It is the absence of such investigation that makes suspect his statement that the working class was not overly concerned about crime. Nor does he support his assertion that "the real object of police activity was the working class, not the lumpenproletariat" (p. 233). Finally, the famous commander of New York's detectives was Thomas, not William, Byrnes.

Haring has presented a book-length exposition of what he is against: capitalism and the mode of policing associated with it. He tells us the kind of police he is for only in a footnote. In contrast to capitalism, under socialism "police activities are directed by the working class and aimed at their class enemies" (p. 262, emphasis in original). It seems as if, no matter what the social system, anyone concerned about liberty had better see to the careful policing of the police.

JAMES F. RICHARDSON
University of Akron

ROBERT MIRAK. *Torn Between Two Lands: Armenians in America, 1890 to World War I.* (Harvard Armenian Texts and Studies, number 7.) Cambridge, Mass.: Department of Near Eastern Languages and Civilizations; distributed by Harvard University Press. 1983. Pp. x, 364. \$25.00.

In *My Name Is Aram*, William Saroyan told of how it was to grow up as an Armenian boy in Fresno, California. Others in autobiographical and fictional accounts have portrayed the warmth and loyalty of the Armenian family, and many writers have focused on the woes and persecutions of the Armenian people in the Ottoman empire. Yet, despite the fact that the Armenian-American community is the most numerous, most affluent, and probably the most influential of all Armenian communities in the dispersion and has already entered its second century, a comprehensive history of that community remains to be written.

Robert Mirak has contributed significantly toward that goal by tracing the history of Armenian immigration and settlement in the United States up to the outbreak of World War I in 1914, by which time there were approximately sixty-six thousand foreign-born Armenians in the country. The product of many years of research and revision, this pioneering study describes the economic and political reasons for emigration, the tribulations of passage to and reception in America, and the settlement patterns, the socioeconomic and political structure, and the concerns of and the tensions within the first generation community.

Unlike most other newcomers from Europe and the Near East, the majority of early Armenian immigrants, mainly bachelors and unaccompanied husbands, did not intend to remain in the United States; rather, they hoped to skimp and save enough money within a few years to return home and free their families from the economic servitude that had shackled the Armenian rural population for generations. The fact that both relatives and homeland suffered under foreign rule agitated the Armenian immigrants, riveting their attention and political activities on developments abroad and contributing to their bitter, sometimes violent ideological rivalries and personality clashes. It was not until several waves of massacre and devastation had engulfed the Armenian lands of the Ottoman empire that most of the first generation gradually accepted the fact that there was to be no return.

Of the five parts into which the book is divided, three are brief: parts 1 and 2 deal with historical background and early emigration and part 5 introduces the subject of acculturation. Part 4 attempts to interrelate the calamitous events in the homeland and the frenzied responses among the Armenians in America. Because this section emphasizes the disruptive feuds among the Armenian political parties, a somewhat exaggerated view of Armenian-American life may be imparted, and although the author tries not to take sides he cannot refrain from severe criticism of the Hinchakian (Marxist revolutionary) party.

The core and major contribution of the book lies

in part 3, "The Terms of Settlement." Here, Mirak is at his best in research, organization, and writing. The focus of this section is on the immigrant in America, his arrival, daily routines at home and at work, his recreational and social outlets, his aspirations and achievements, and his attitudes toward family, church, and nation. Differences in outlook between Armenian Protestants and the majority that still adhered to the national Apostolic Church are assessed, and some interesting comparisons are made between Armenian and other immigrant groups in numbers, education, employment, income, and intermarriage. Mirak succeeds in capturing the particular regional flavor yet also the overriding unifying features of the various communities, including those of the factory and foundry towns of New England, the metropolitan centers of the Middle Atlantic states, the higher-wage-paying cities of the Midwest, and the great agricultural expanses in the San Joaquin Valley of California. His fascinating descriptions of streets, factories, clubhouses, and other sites intrinsic to the history of the early communities cannot but give rise to bittersweet nostalgia to those old enough to remember them. Some vintage photographs add a visual dimension to the book.

Mirak is confident in dealing with the central themes of his study but reveals weakness in providing precise historical background. The many minor errors in detail do not alter the soundness of his findings on the larger issues, but it is regrettable that the manuscript was not read by a competent specialist in Armenian history before publication. In the brief historical section, for example, Mirak reiterates several outdated assumptions and makes a series of inaccurate statements, among them that the *millet* system safeguarded the Armenians from "intermarriage, conversion, and assimilation" (p. 6); that Marcel Leart was a distinguished French demographer (p. 15); that Dersim is south of Kharpert (p. 17); that Russia conquered Turkish territory in 1822 (p. 29); that Bedros Tourian was a Russian-Armenian intellectual (p. 29); and that there was a "Janasarian Academy" in Tiflis (p. 29) (there was a Nersesian academy in Tiflis and a Sanasarian academy in Erzerum). Errors of this type continue with less frequency in the subsequent parts. In discussing the history of the Armenian Church, for example, Mirak states (p. 181) that the Armenians broke with the Roman and Greek churches in 491 and were regarded as heretics "for avowing that Christ had a single, divine nature." Actually, the Armenian Church itself condemned that position as heresy centuries ago.

Robert Mirak has broken new ground with this book. There is still much to be learned and written even about the period covered. This study will provide today's large Armenian-American commu-

nity with a look back at its origins and early years and will be an important contribution to those interested or engaged in ethnic studies. The story remains unfinished, and it is to be hoped that Mirak and others will carry the history forward.

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DEWEY W. GRANTHAM. *Southern Progressivism: The Reconciliation of Progress and Tradition*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1983. Pp. xxii, 468. \$34.95.

In the four decades since Arthur S. Link first asserted the existence of a Progressive movement in the South, an increasing number of historians have examined various aspects of reform in Dixie. The contributions of C. Vann Woodward, George B. Tindall, and Sheldon Hackney were especially significant in stimulating inquiry about the many ways in which southerners labored to achieve social justice, economic regulation, and moral uplift. Dewey W. Grantham's important volume synthesizes the monographic literature and the abundant primary source materials into a study that describes southern progressivism as "perhaps the most significant venture in social reform undertaken thus far in the twentieth-century South" (p. 422).

Grantham's command of the sources is impressive. He has read or consulted, it would seem, every scholarly article that bears on Progressive reform in the South in this period, and he has an equally comprehensive knowledge of the relevant master's theses and doctoral dissertations written in the past thirty years. No one person could visit and see all the pertinent manuscript collections in the region, or outside it, but Grantham has been to an awesome number of repositories. From these primary sources he has derived numerous apposite and interesting quotations as well as a solid grasp of the issues and personalities of the time. Based on its abundant notes and excellent bibliographical essay alone, this would be a book from which historians could profit greatly.

Judicious analysis and persuasive insights have accompanied Grantham's inclusive research. His narrative is always balanced and fair, and he is generous in his evaluation of the publications of his co-workers in the field. He is equally even-handed with the historical actors whom he examines. Cognizant of the limitations of their thought on racial issues, for example, he does not lecture southern reformers for their manifest failings. Instead, he shows how southerners rationalized racist tactics against black Americans as a positive change in regional politics. The ensuing contradiction be-

tween racial oppression and the ostensible democratic impulses of the Progressives becomes even more striking for Grantham's calm account of its development.

In some instances Grantham's detachment and restraint do take the edge off a few of the important personalities of the era. He treats James E. Ferguson of Texas with more kindness than "Farmer Jim's" shabby business practices and demagogic attacks on higher education warrant, and he perpetuates the inflated reputation of James S. Hogg as an economic reformer. The inflammatory language of James K. Vardaman toward blacks is well described. The similar activities of Tom Watson, Ben Tillman, and Theodore G. Bilbo in this regard are touched on in muted tones. As a result some of the violence, passion, and obsession of southern politics before 1920 is absent.

A separate chapter is devoted to the movement to restrict the sale of alcohol and prohibit its use in the South. Here Grantham reflects the work of numerous scholars who have been quietly reestablishing the liquor issue as an important aspect of the political debates in the early twentieth-century South. His conclusion that prohibition was a "major feature of southern progressivism" (p. 161) accurately fixes the place of this cultural reform on the regional agenda. Grantham still evidences some surprise that southerners devoted so much attention to alcohol and its effects, but he understands that the ramifications of the liquor problem were so large that concern with it was inescapable. The care and thoroughness given to prohibition in this book underscores the need for a comprehensive treatment of the adoption of prohibition nationally. Historians and writers in other disciplines have done useful work on many aspects of the issue, but no satisfactory synthesis has yet appeared.

The impact of the First World War on the South also deservedly receives a chapter of its own. The European conflict hastened the achievement of Progressive campaigns in the region and further intensified southern linkages with the rest of the country. Woman suffrage, prohibition, child labor legislation, and public health advanced during the war. The uglier side of the nation's involvement is somewhat played down, and the amount of popular support for the war in the South may be overstated. State councils of defense, often zealous and intemperate bodies, did carry on "persecution of 'slackers' and 'radicals'" (p. 390) with an ardor that sometimes intimidated or injured those in their path. State politicians shamelessly used the war hysteria against vulnerable opponents, as William P. Hobby did against Ferguson in Texas in 1918. The Wilson administration's campaign the same year against southern lawmakers it deemed unfriendly is given only passing scrutiny. The elevated level of public

passions that marked southern politics in 1917–18 probably had something to do with the waning of the Progressive spirit by 1920.

Students of the South and American history generally will find many other subjects given abundant coverage. There are lucid discussions of economic regulation, the campaign to improve education, and the effort to improve agriculture. Penal reform, public health, child labor, conservation, feminism, and the transformation of urban government are examined carefully and well. The several chapters that review southern politics in the lower South, upper South, and Southwest respectively are notable for their tight organization and adroit summaries of issues and leaders. Grantham has performed a monumental labor of synthesis and analysis that will not have to be done again. The University of Tennessee Press has served his text well with useful tables and interesting photographs. Would that their proofreading attained the same standard. This book will long be regarded as the definitive treatment of Progressivism in the South.

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HOWARD JABLON. *Crossroads of Decision: The State Department and Foreign Policy, 1933–1937*. Lexington: University Press of Kentucky. 1983. Pp. ix, 182. \$18.00.

This brief, 140-page account of the role of the State Department in setting foreign policy in the first Franklin D. Roosevelt administration is a fine introduction to the subject. Writing from a consciously "realist" perspective Howard Jablon addresses these subjects: recognition policy, neutrality legislation, attitudes toward Asia, collective security, and responses to the Spanish Civil War and the Italian invasion of Ethiopia. Throughout he argues that the professional diplomats of the State Department, despite self-congratulations, were no more astute than their political superiors or members of the general public. Like most articulate Americans, they embraced legalistic solutions for problems that demanded greater imagination. In the end they failed to offer their political superiors the sort of sophisticated advice they believed themselves capable of supplying.

Many of the details in *Crossroads of Decision* are well known, but Jablon has provided a fresh framework by concentrating his attention on the State Department's contribution. He is strongest in his discussion of the precise role of experts in shaping the department's response to congressional efforts at neutrality legislation. Despite claims by the diplomats that they were alert to the dangers of removing

the United States from European quarrels, Jablon points out that department experts were as enthusiastic as other advocates for neutrality laws.

The book's dates, 1933–37, are a trifle misleading. Jablon's focus is indeed the first four years of FDR's administration, but the argument rests on the premise that New Deal diplomats operated no differently from the way they behaved at the end of the Hoover administration. He repeatedly refers to conduct during Henry L. Stimson's tenure at the department. A more accurate subtitle might have indicated that *Crossroads of Decision* covers the period from the onset of the Depression until the "quarantine" speech of October 1937.

Greater attention to bureaucratic processes would also have made this a better book. Although Jablon covers the State Department, he could have offered more analysis of the ways in which policy percolated to the department's principal officers. He goes a long way in this direction while treating the origins of neutrality legislation, and it would have been worthwhile to pursue this approach while writing about other issues.

Still, Jablon has provided a good summary of the State Department's actions in the Depression. Recent scholarship has exploded the myth of professional omniscience, but if anyone took diplomats' claims to wisdom at face value, *Crossroads of Decision* would set them straight. It is a good and wise book.

ROBERT D. SCHULZINGER
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NANCY J. WEISS. *Farewell to the Party of Lincoln: Black Politics in the Age of FDR*. Princeton: Princeton University Press. 1983. Pp. xx, 333. Cloth \$32.50, paper \$12.50.

Those familiar with the previous scholarship on race relations and black history by Nancy J. Weiss will not be surprised by the excellence of *Farewell to the Party of Lincoln*. Weiss has again written a masterful historical study characterized by originality of interpretation, depth and breadth of research, and elegance of composition. It is a work to be read with profit and pleasure by even those who may disagree with Weiss about certain particulars.

The central theme of the study is how and why Afro-Americans developed an overwhelming commitment to the party of Roosevelt and the New Deal. But the book is far more than a quantitative analysis of black voting behavior. Skillfully summarizing recent historical treatments of blacks in the 1930s and adding insights and information based on fresh research, especially the three-score oral history interviews that Weiss conducted, *Farewell to the Party of Lincoln* depicts the manifold activities of black organizations and politicians during the Great

Depression, the battles for antilynching and anti-poll tax legislation, the crucial role of Eleanor Roosevelt, the functions and value of the Black Cabinet, the racist tenor of the times, and the impact of New Deal programs and policies on black Americans. The imposing tapestry of interrelated subjects richly woven by Weiss makes *Farewell to the Party of Lincoln* a most welcome and significant contribution to the growing scholarship on blacks and the New Deal. The sensitive and eloquent account of the Marian Anderson concert at the Lincoln Memorial in 1939 is alone worth the cost of the book.

Reviewers, of course, have the right to quibble. There are some factual inaccuracies. For example, Rexford G. Tugwell headed the Resettlement Administration, not the Farm Security Administration (p. 49); and President Roosevelt endorsed the movement for repeal of the poll tax in Arkansas, not in Arizona (p. 250). In a number of key areas, moreover, Weiss uses statements made in the 1940s to characterize attitudes held in the 1930s, not allowing for the ways in which the experience of World War II, particularly the race riots of 1943, altered perceptions (for example, pp. 129, 132, 253). In addition, Weiss relies much too heavily on the nearly half-century-old memories of New Dealers who were not involved in civil rights matters (like Thomas Corcoran) and of blacks in leadership positions today who were young children during the New Deal (such as Kenneth B. Clark and Carl Stokes), as well as on relatively obscure Republican newspapers (like the *Iowa Bystander*), to hammer home the point that Roosevelt and the New Deal were not concerned with civil rights. Most fundamentally, the thesis of Weiss—that blacks voted Democratic because of their economic concerns and despite Roosevelt's record on race—remains questionable. Black voters in the 1930s were not polled on why they cast their ballots as they did; any attempt to infer that from voting statistics must acknowledge, as Weiss does, that the “numbers involved are small; the election units studied necessarily comprise only a fraction of the black electorate in the respective cities and an even smaller fraction of the black electorate nationwide” (p. xvi). There is simply not sufficient evidence to conclude that black voting behavior in the 1930s differed significantly on the basis of economic status. What we do know for sure is that the mass of blacks, despite extreme privation, joined the New Deal coalition later than did poor whites, and by 1940 they were far more enthusiastic about the New Deal than other poor Americans. Much of the evidence on black attitudes in the 1930s, moreover, supports the view that despite the shortcomings of the New Deal, on both racial and economic matters, most blacks realistically and pragmatically supported FDR because they received much more from the New Deal than blacks were

accustomed to getting in both economic and racial assistance.

The historical debate on this subject will continue. *Farewell to the Party of Lincoln* has stimulated it and enriched it. The perceptions and judgments of Weiss will have to be reckoned with for many years to come.

HARVARD SITKOFF

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STEVE NEAL. *Dark Horse: A Biography of Wendell Willkie*. Garden City, N.Y.: Doubleday. 1984. Pp. ix, 371. \$17.95.

As one-campaign-losing presidential candidates go, Wendell Willkie has attracted more than his share of biographies and scholarly studies, and justifiably so. His career involved a bundle of almost flamboyant paradoxes. He was a Wilsonian liberal turned corporation lawyer, a leader of the public utilities industry in its battle against the New Deal who supported social security and the rights of labor unions, a Democrat and card-carrying member of Tammany Hall turned Republican presidential candidate, and the nominal leader of the mostly isolationist Republican party who supported a United Nations organization. During his short and turbulent career in the public limelight, his influence in uniting Americans to prepare for entry into World War II and for responsibility for peace in the postwar era may well have been second only to that of FDR himself. In a sense Willkie never left home, except that he went his hero, Woodrow Wilson, one better by also becoming an early and outspoken advocate of equal rights for blacks.

All of this, combined with Willkie's own effervescent personality and free-wheeling life style, is the stuff of high drama, and Steve Neal, a political writer for the *Chicago Tribune*, brings considerable talent for straightforward storytelling to this brief account. It would not be quite fair to call *Dark Horse* a popularization, though it is by no means a definitive account. Neal has, to be sure, done extensive work in the Willkie papers as well as in the other pertinent archival sources. He has also interviewed many of those involved in the Willkie phenomenon and examined most of the relevant secondary works.

For all that, it is surprising how little Neal is able to add to Ellsworth Barnard's fine and much more extensive account, *Wendell Willkie: Fighter For Freedom* (1966). Particularly disappointing is Neal's treatment of Willkie's early years as well as the highly complex story of Willkie's struggle as head of the Commonwealth and Southern holding company with the TVA. Indeed, of the book's 324 pages of text, only 45 deal with Willkie's first forty-seven

years, a treatment totally inadequate for coming to grips with the personality of the man who was Willkie.

Neal's interests are almost exclusively in Willkie's last five years, from 1939 to 1944, and, as in the case of previous accounts, a strongly sympathetic bias for Willkie and all he stood for is evident. But, if there is no challenge to the earlier interpretations (and little analysis of any kind), Neal does shed fresh light on Willkie's relationship with Irita Van Doren, his rather risky captivation by and, perhaps, dalliance with Madame Chiang Kai-shek, his politically suicidal detestation of Thomas Dewey, as well as many other minor but new and interesting details.

This account will undoubtedly find a place on the shelves of high school and undergraduate college libraries, but serious students of the political scene in the 1940s should not neglect it.

BERNARD F. DONAHOE
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Holy Cross Junior College

WILLIAM T. Y'BLOOD. *Hunter-Killer: U.S. Escort Carriers in the Battle of the Atlantic*. Annapolis: Naval Institute Press. 1983. Pp. viii, 322. \$19.95.

Naval historians are too often accused of being preoccupied with the "guns-and-boats"—that is, the battle narratives—aspects of their field. Such accusations are borne out in works like William T. Y'Blood *Hunter-Killer*. Very good indeed on operational history, it presents in purely narrative fashion the offensive by the U.S. Navy (and some British) antisubmarine escort carrier groups against Admiral Karl Dönitz's U-boats during the final two years of the Battle of the Atlantic, spring 1943 to V-E Day. The author's easy writing style, accompanied by superb photographs of the U-boats under attack, will satisfy the most ardent naval buff. Each battle between HUK group and its prey is told in fascinating detail that, however, borders on the repetitive, such being the nature of the campaign.

New to this operational history is the heretofore undisclosed impact of the broken German "Enigma" codes—knowledge of which enabled the Allies to divert their convoys around wolf-pack concentrations and to concentrate the HUK groups at refueling rendezvous points of the "milch cow" submarines and their fuel-hungry charges. Thus, Allied naval forces destroyed 63 U-boats directly and 30 indirectly because of the Enigma information. Although antisub forces, primarily British and Canadian, had virtually checked the U-boat onslaught by spring 1943, it was the U.S. HUK groups that broke away from the defensive escorting of convoys to operate offensively and to defeat decisively Hitler's underwater fleet. They sank only 53 boats and

captured one—7 percent of the 785 sunk—but completely upset Dönitz's submarine strategy and thus secured the vital sea lanes supporting the liberation of Western Europe.

Unfortunately, the book is not analytical. Instead of well-integrated analyses of technological and tactical changes, key innovations are unsystematically and undramatically dropped into the text almost in passing: the German acoustic torpedo, anti-aircraft armament, the snorkel breathing device, and the American aerial-homing torpedo called Fido, rockets, the sonobuoy, airborne radar, destroyer escorts, magnetic airborne detection gear, and aircraft searchlights (the latter unobtrusively introduced deep inside a long quotation).

Tactical innovations are treated in a very unsatisfactory manner, the one chapter discussing them being disjointed, even jumbled. Instead of wasting chapters describing the escort carriers' support of the landings in North Africa and southern France in basically non-HUK roles, the author might have used the space more profitably to tell how the important Antisubmarine Development Unit of the Atlantic Fleet applied the lessons learned to HUK operating directives, and how the antisub training schools developed these new tactics. Excellent tactical diagrams and charts are not discussed in any of the accompanying text.

Citation of supporting evidence leaves something to be desired. Use of the German Naval Command War Diary is admirable, but we are not told if it was used in translation. P. K. Lundberg's key work should have been cited as the Ph.D. dissertation on which the too-often quoted volume in Samuel Eliot Morison's epic history was based. Worst of all is the author's irritating habit of using quotations without attributing them to anyone, except in the notes, the kind of writing that gets undergraduates into trouble in their term papers. And the index lists only proper names, thus seriously compromising its usefulness.

Still, *Hunter-Killer* fills a gap in World War II historiography by moving the converted tanker and "Kaiser-coffin" escort carriers and their supporting destroyers to the center stage where they belong.

CLARK G. REYNOLDS
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HAIM GENIZI. *American Apathy: The Plight of Christian Refugees from Nazism*. Ramat-Gan, Israel: Bar-Ilan University Press. 1983. Pp. 411.

Approximately 30 percent of the refugees entering the United States from Nazi-dominated Europe during 1933–45 were non-Jews. Haim Genizi concentrates on American attitudes toward Christians seeking asylum, official restrictions on immigration,

and private efforts to facilitate relief in Europe, entry into the United States, and resettlement here. In particular, he details the successes, failures, and ethical lapses of some of the most prominent organizations (there were seven hundred by 1941) concerned with refugee problems. These include groups sponsored by Quakers, Unitarians, Roman Catholics and other denominations, interdenominational alliances like the American Christian Committee for German Refugees, such nonsectarian efforts as the Emergency Relief Committee, and specialized societies aiding scholars, physicians, and musicians.

On the whole, Genizi's research is thorough, his writing clear, and his judgment sound. While criticizing the widespread and "alarming" (p. 73) obliviousness to Nazi persecution, he praises the accomplishments of a "relatively small group of devoted Christian humanitarians" (p. 338). In particular, he applauds the American Friends Service Committee and the Unitarian Service Committee; however, the Federal Council of Churches and YMCA, he notes, illustrate American apathy. Genizi ably considers President Roosevelt's evasiveness, psychological stress felt by so-called "non-Aryan Christians" (p. 21), interdenominational rivalries, and the "Fifth Column scare" (p. 191) that heightened hostility toward refugees. Among his interesting asides, we learn that the United States' "Vichy gamble" allowed the Emergency Relief Committee to operate in unoccupied France where, through cunning and diplomacy, it saved such prominent figures as Marcel Duchamp, Marc Chagall, and Franz Werfel. *American Apathy* deals carefully with the complex, controversial question of relations between Nazism and the Roman Catholic Church (although Genizi underestimates diversity within American and world Catholicism). Sometimes the author's quest for balance obscures his meaning. It is hard to tell from Genizi's conflicting statements whether he thinks James G. McDonald, the League of Nations high commissioner for refugees, a weak incompetent or an able man who made the best of a bad situation.

Despite its subtitle, almost half of *American Apathy* treats Jewish efforts to flee Nazi-dominated Europe. Drawing on standard works by Henry Feingold, David Wyman, and Saul Friedland, Genizi reviews abortive attempts to relocate Jews en masse, notes the prevalence of American anti-Semitism, and criticizes bigots in the State Department and at large. Instead of retelling this familiar story, he might have used his pages more wisely by comparing problems faced by Christians and Jews. If Americans were as hostile to Christian immigrants as Genizi suggests, then the complementary aversion to their Jewish counterparts looks less like the product of anti-Semitism in particular and more like the product of nativism in general.

This failure to compare the difficulties facing

Christians and Jews signals a deeper problem, the author's preference for narrative at the expense of analysis. For instance, though Genizi shows that American Christians responded variously to Nazi persecution abroad, he fails to relate these divergent responses to either denominational social bases or theological positions. Nor does he say much about the emigrants' plight after they reach the United States. Despite these limitations, *American Apathy*, an administrative history with a moral, is a useful book on an important subject.

LEO P. RIBUFFO

George Washington University

IMANUEL WEXLER. *The Marshall Plan Revisited: The European Recovery Program in Economic Perspective*. (Contributions in Economics and Economic History, number 55.) Westport, Conn.: Greenwood. 1983. Pp. xi, 327. \$35.00.

This book is a serious effort to evaluate the economic aspects of the Marshall Plan and related American efforts to put Western Europe on a self-sustaining basis during the period between 1947 and 1952. Imanuel Wexler identifies four American policy goals: a strong production effort, internal financial stability, trade liberalization, and economic integration. As far as the production effort was concerned, he concludes that restrictions embedded in the Foreign Assistance Act of 1948 and in the criteria by which American aid was allocated actually impeded efforts to maximize production in Europe. So, too, did the shift in American aid from civilian to military production after the outbreak of the Korean War in 1950. Nevertheless, European productivity increased during the course of the European Recovery Program (ERP) and, in the industrial sector at least, original production targets were achieved, even surpassed, by the end of 1951. Policy makers in the Economic Cooperation Administration (ECA), the American agency charged with administering the recovery program, were less successful in promoting internal financial stability in the participating countries. The progress made on this front during the first two years of the Marshall Plan quickly evaporated once the Korean War and the rearmament program led to new shortages and inflationary pressures on the continent.

Similar pictures emerged in the areas of trade liberalization and economic integration. Wexler traces the development of various intra-European payments plans, culminating in the European Payments Union of 1950, through which ECA sought to eliminate the payments barriers to trade liberalization on the continent and forge a single, integrated market. By the end of the ERP period, these efforts had helped to promote a substantial expansion of trade among the European countries and

between them and the rest of the world. Yet these gains fell short of putting the participating countries on a self-supporting basis and creating a single European market, in large part because of British opposition to the sacrifices of national sovereignty and self-interest that true integration entailed. In the end, Wexler concludes, the greatest achievements in this area, particularly the European Coal and Steel Community and the Common Market that followed, stemmed less from American than from European initiatives and took shape outside the framework of the Marshall Plan. In spite of these assessments, Wexler's final judgment is positive. It was unrealistic, he argues, to believe that American policy goals could be attained in a four-year period. Despite its shortcomings, moreover, the Marshall Plan laid the foundation for continued economic progress by the Europeans themselves.

Wexler's survey would have been enhanced had he explored the archival material available in Britain, the largely untapped records in ECA's telegram file, the substantial holding of the Treasury Department, and the decimal files of the State Department. Such research would have enabled him to develop more fully the serious disputes among American policy makers and between them and their European associates over the purpose of the recovery program. As it is, his volume focuses narrowly on ECA's view and often amounts to a narrative account whose economic perspective does not tell the reader enough about policy formulation and the give-and-take of the diplomatic process. Nevertheless, Wexler's economic assessments seem reasonable and the above criticisms should not detract from his accomplishment in this area.

MICHAEL J. HOGAN
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DONALD R. RAICHLIE. *New Jersey's Union College: A History, 1933-1983*. Cranbury, N.J.: Fairleigh Dickinson University Press or Associated University Presses, London. 1983. Pp. 270. \$20.00.

ALVIN W. SKARDON. *Steel Valley University: The Origin of Youngstown State*. Youngstown, Ohio: Youngstown State University. 1983. Pp. xi, 288.

To the hundreds of published histories of colleges and universities have been added the official histories of a community college in New Jersey and an urban university in Ohio. Each of these histories has limits. Both illustrate the diversity of origins and development of American higher education.

This Union College (enrollment 1982, 9,531) is the community college of Union County in Eastern New Jersey. It began as a New Deal work relief project in 1933, with classes held after hours in a

high-school building. It was seen at its beginning as a liberal arts "transfer" school. Its original faculty were academics; five of the seventeen had Ph.D.'s and eight others had masters degrees. In 1936 federal funds were discontinued and Union continued as a tuition supported junior college, which in 1941 was housed in a venerable rented mansion. Nearly bankrupt in 1944, it was buoyed up by the GI flood, and benefited from tax support beginning in 1965. By 1983 it had become quasi-public. Donald R. Raichle sees Union's history as a chapter, or at least a case study, in the development of the junior or community college.

The university that is now Youngstown State (enrollment 1980, 15,351) began with classes offered by the YMCA about a century ago. The utilitarian, practical, and vocational were emphasized. The first instructors were local businessmen, engineers, professionals, and high-school teachers. At one time or another classes were offered in elementary and high-school subjects, telegraphy, auto repairs, business subjects, law, education, and engineering. Youngstown College's "pulling away" from the YMCA was without acrimony and was completed in 1944. This history, unfortunately, ends on the eve of Youngstown's annexation to the state system in 1967. For Alvin W. Skardon, Youngstown State's history is viewed as a part of urban history more generally; this seems more satisfactory, but circumstances differ.

Geographic distance and a half-century's separation in time of founding dictated differences in the schools. Furthermore, the YMCA and New Deal backgrounds spawned different kinds of schools. But there are similarities. New Jersey was notorious for its lack of support for public education. Youngstown seemed of the same mind; the high school there seems to have been private until 1918. Both schools were shaped by accreditation agencies, perhaps an analogue to the ways in which public education was molded into the "one best system." Each, delivering schooling at low cost, grew during the Depression.

Raichle and Skardon have written histories that are largely concerned with administration, dwelling on the actions of trustees, directors, deans, and presidents—perhaps this old convention should have been muted. Many details are unintelligible to the outsider. Where, for instance, is Lustig's Shoe Store, or Winfield Scott Motel? Presumably these books will bring satisfaction to administrators and pleasure to alumni. For historians the reward will be much less. Still, they may serve as bits of a yet-to-be-composed mosaic of the history of American higher education.

H. WARREN BUTTON
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MICHAEL W. SEDLAK and HAROLD F. WILLIAMSON. *The Evolution of Management Education: A History of the Northwestern University J. L. Kellogg Graduate School of Management, 1908–1983*. Urbana: University of Illinois Press, for Northwestern University. 1983. Pp. x, 202.

Financial pressures in 1933–34 caused university trustees at Northwestern and Chicago to plan to consolidate their operations on three campuses—Evanston (undergraduate), Hyde Park (graduate), and downtown Chicago (professional). Northwestern's School of Commerce (founded in 1908) then favored such a merger, which had it gone through would have significantly heightened the mounting rivalry among American business schools. But since the scheme never materialized, Northwestern's commercial school proceeded in its gradual, sometimes tortuous, evolution toward the distinctive program in management education that this Diamond Jubilee history celebrates.

Harold F. Williamson's work with Payson S. Wild on *Northwestern University: A History, 1850–1975* (1976) provided valuable background in archival sources and broad coverage for this new study. Michael W. Sedlak's doctoral dissertation of 1977 ("The Emergence and Development of Collegiate Business Education in the United States, 1881–1974: Northwestern University as a Case Study") was even more vital to the collaborative effort. What emerges is a well-documented account that seeks to be both an official institutional history and an illuminating analysis of the evolution of commercial/business/management education under the university's auspices.

As institutional history, it dutifully tries to cover a bewildering array of administrative and academic activities—graduate and undergraduate, evening and day, downtown and in Evanston. By breaking up the story into six chronological chapters, each centered about an important dean with a particular approach to the school's problems, the authors impose a certain organization on the data. Within chapters, furthermore, they set up brief sections on different aspects of the wide-ranging programs. Thus the second chapter ("School of Commerce: Growth and Service, 1919–1939") describes Dean Ralph E. Heilman's special approach and then takes up sixteen topical sections such as "Bachelor of Science in Commerce," "Medill School of Journalism," "Bureau of Business Research," "Business Ethics," and finally "Finance."

Through these devices the volume does cope with a central problem of every institutional history. Yet some operations (on the Chicago campus especially) slip through the cracks, whereas personnel and curricular changes become so detailed after 1965 as to put off the general reader. What may work for

administrative developments, moreover, does not capture the spirit of the classroom or of student life before recent times. Most seriously, the index will be difficult if not impossible to use: general categories such as "Curriculum" or "Enrollment" are not broken down; an important president, J. Roscoe Miller, is cited for only two of eight pages where he appears; Abbott Hall is pictured as well as discussed but never cited; there are no entries for "Trustees" or "Evanston" or "Computer," let alone "Business history" or "History, business."

Within each chapter the authors also advance their main theme: the evolution of management education. To be quite sure that it is understood amid all the detail, they conclude with a six-page "Retrospective." Taken together with the introduction, which briefly describes earlier efforts at business education, this conclusion effectively summarizes how Northwestern's School of Commerce came to be the prestigious J. L. Kellogg Graduate School of Management. Questions inevitably remain, particularly on some vital appointments to dean, yet forces at work in this fascinating evolution here find satisfying summation.

G. WALLACE CHESSMAN
Doane Library,
Denison University

JAMES SLOAN ALLEN. *The Romance of Commerce and Culture: Capitalism, Modernism, and the Chicago-Aspen Crusade for Cultural Reform*. Chicago: University of Chicago Press. 1983. Pp. xv, 336.

Modernism has, for the most part, presented itself as an oppositional culture, and its commentators generally have shared this understanding. Now, in the age of "postmodernism," somewhat more critical and genuinely historical approaches to modernism are being developed. James Sloan Allen's book, *The Romance of Commerce and Culture*, is an important exploration of insufficiently noticed connections between modern art and the culture of consumerism.

The geography of Allen's story, beginning in Chicago and ending in Aspen, Colorado, seems at first glance to put it on the periphery. But in fact he offers a narrative of a central episode in American cultural history, one both symbolic of larger cultural movements and a potent force in its own terms in shaping upper-class culture in the United States in the 1940s and 1950s.

The central figure in this story is Walter Paepcke, the founder of the Container Corporation of America. Tutored, first by his wife, Elizabeth (née Nitze), and then by Lazlo Moholy-Nagy, Herbert Bayer, and Walter Gropius, Paepcke grasped a natural alliance of artistic modernism and the imperatives of consumer capitalism. The result was the promotion

of "good design" in the interest of a positive "corporate image" and of a direct appeal to the subjective desires of the consumer.

For Moholy-Nagy good design pointed toward cultural reform, and after Moholy-Nagy's death, Paepcke embraced a vision of cultural reform articulated by Robert M. Hutchins and Mortimer Adler. The arts and humanities, according to this rather unlikely threesome, needed to be pressed on Americans in order to supply values in a scientific age, to establish American democratic values and cultural ascendancy in the contest of freedom and communism, to refine excessive materialism rampant in America without challenging a higher sort of consumption, and, finally, to elevate, discipline, and integrate American upper-class culture. This program—and the appeal of Aspen as a real estate investment—led to the development of the town as a resort for the "better class" and to the founding of the Aspen Institute for Humanistic Studies. After a brief period of uncertainty about the program, Henry Luce persuaded Paepcke to bring together the various segments of the American elite for discussions on the arts and humanities, and in 1951 this became the program. If leaders of business, government, labor, and culture talked at Aspen, they would overcome their estrangement and orchestrate an American consensus.

It is an important story in its own terms, but in Allen's quite detailed, although always urbane, intelligent, and well-written book, we have an unusual and exceptionally suggestive perspective on the American upper class and its culture.

Allen recognizes that his story is open to two radically different interpretations. One can call it progressive, the story of a movement that extended the appreciation of the arts generally and modernism particularly. Or it can be seen as a form of cooptation or corruption of artists and intellectuals, limiting their criticism of the basic premises of consumer capitalism. Allen insists, correctly I think, that it is not a matter of choosing one or the other, or even a bit of each, but rather that both of these views are in their entirety valid.

THOMAS BENDER
New York University

H. CRAIG MINER. *The Rebirth of the Missouri Pacific, 1956–1983*. College Station: Texas A&M University Press. Pp. xx, 236. \$19.50.

This handsome volume reviews the recent corporate history of the Missouri Pacific, a railroad that emerged from a long twenty-three-year receivership in 1956. A century before, in 1852, the Pacific Railroad of Missouri had run the first locomotive west of the Mississippi over the freshly laid rails of a

five-mile line west of St. Louis. By 1956 the Missouri Pacific was operating about ninety-five hundred miles of line out of St. Louis. Western lines ran to Kansas City, Omaha, and Pueblo, Colorado. To the southwest the system served Little Rock, Memphis, San Antonio, Houston, and Brownsville. The road also included the eighteen-hundred-mile Texas and Pacific with a line from New Orleans via Dallas and Fort Worth to El Paso. The Missouri Pacific owned about 75 percent of the capital stock of the T. and P. When the Missouri Pacific emerged from its receivership it was plagued by the existence of two types of common stock with the inferior B stock largely held by the Alleghany Corporation. Shortly after the reorganization, William Marbury, a St. Louis financier, obtained control of about a quarter of the dominate A stock of the Missouri Pacific. Marbury quickly lured Downing Jenks away from the Rock Island to become president of his road. By 1966 Marbury and Jenks had obtained control of the Chicago and Eastern Illinois, giving the Missouri Pacific a valuable line from St. Louis to Chicago. It took longer to end the battle between the rival A and B stockholders, but a recapitalization plan approved in 1974 finally freed the railroad from any Alleghany Corporation control.

With the recapitalization complete, the next step was the full merger of the Chicago and Eastern Illinois and the Texas and Pacific into the Missouri Pacific system. During the 1960s and 1970s Downing Jenks had been emphasizing a management training program throughout the road and the intensive recruiting of interested college graduates. Jenks also insisted on adopting both innovations in service and new technology in all phases of railroad-ing. An early change was the standardization of all motive power as well as shop and repair facilities. The Missouri Pacific was a major promoter of piggyback service, containerization, and unit trains. New freight cars to meet the specific needs of customers were also placed on the line. Computers became available for railroad use about the same time that the road had escaped from receivership. Jenks and his top officials successfully used computers for the purchase and inventory of materials, faster and more reliable communication, and improved availability and distribution of freight equipment. All Missouri Pacific passenger service had been drastically curtailed prior to the appearance of Amtrak. By the late 1970s the revitalized Missouri Pacific was ready to join the "urge to merge" movement that was so prevalent throughout the railroad industry. Early in 1980 it was announced that the Missouri Pacific would merge with the strong and diversified Union Pacific. Some observers claimed the proposed twenty-two-thousand-mile giant system would be like combining the Dallas Cowboys with the Pittsburgh Steelers. The merger was ap-

proved by the Interstate Commerce Commission in September 1982.

This railroad history is unique in that the story is one of rebirth and reform rather than an account of the original projection and construction of a rail system. H. Craig Miner was fortunate to find that the Missouri Pacific had an extensive and well-maintained collection of company records. Although the use of these company archives make up the bulk of the volume, the author based much of his story on innumerable interviews with dozens of railroad officials and lawyers. The lively text is enriched with many photographs, three maps, and a brief index. Some readers may wish that more detailed figures had been included on the expanding traffic and revenue of the road. *The Rebirth of the Missouri Pacific* is a definitive and well-researched history of the revitalization of a major American railroad achieved in the last generation.

JOHN F. STOVER
Purdue University

CATHERINE A. BARNES. *Journey from Jim Crow: The Desegregation of Southern Transit*. (Contemporary American History Series.) New York: Columbia University Press. 1983. Pp. xi, 313. \$28.00.

With more recent attention paid to discrimination in housing, employment, and education, Americans have tended to overlook the serious disabilities that blacks once faced in transportation and voting. *Journey from Jim Crow* sets the record admirably straight on travel, just as another series volume, Steven Lawson's *Black Ballots*, did for voting rights in 1976.

Catherine A. Barnes has employed an impressive range of sources, including the customary manuscript collections, oral interviews, government publications, and secondary materials. Especially crucial to this topic, however, was her considerable immersion in federal court dockets and rulings and the decisions of the Interstate Commerce Commission. The preponderance of articles from the 1950s and 1960s in her bibliography, and the fact that no citations more recent than 1974 appear from popular, academic, or legal journals, may confirm the tendency of laypersons and specialists alike to disregard segregated transportation. Fortunately, Barnes has rescued the topic with a highly readable volume that will benefit general and scholarly audiences.

The author opens with a chapter on the legal and extralegal resistance to black rights following the promising days of Reconstruction, which underscores not merely the persistence of racism but the gradual and complicated processes required thereafter to remedy discrimination in transportation. Reform was all the more difficult because of Ameri-

ca's system of multiple jurisdictions, whereby local, state, and federal officials could absolve themselves of responsibility by directing complainants to another, any other, level of government. This was particularly prevalent in transportation, since some travel was strictly local, some interstate but still within regional bounds, and some across sectional boundaries with differing laws and customs on race. Should the state or federal government provide redress? Should one appeal to the courts, an executive branch, or a regulatory agency such as the Interstate Commerce Commission? Would the Fourteenth Amendment with its guarantees of equal protection or the Constitution's interstate commerce clause bring recalcitrant states and carriers into line? Is bus travel basically different from the more varied services available on the rails? What of stationary facilities like restrooms, restaurants, and waiting rooms, especially when the carriers merely lease rather than own them? Can private companies and their employees establish and impose their own regulations?

Barnes deals with these complications in a systematic and convincing fashion. She examines at length such Supreme Court decisions as *Mitchell* and *Henderson* (rail passenger cases, 1941 and 1950, respectively), and *Morgan* and *Gayle* (interstate and local bus cases, 1946 and 1956, respectively), and the 1955 ICC rulings (*NAACP* and *Keys*) to desegregate terminal facilities. If the closing three chapters, which focus on the early 1960s, seem a bit hurried and in need of additional, contextual development, the reader, nonetheless, still perceives that the timing and pace of desegregation depended on two elements: the extent and quality of federal involvement and the pressures generated by civil rights activists (whether through litigation, politics, or civil disobedience). Perhaps those who talk so glibly about "getting the government off our backs" in the 1980s should ponder those historical realities.

For accuracy, since so many of us in the field make this mistake, it should be noted that FEPC, the Fair Employment Practice Committee, contains no plural words in its title.

ROBERT L. ZANGRANDO
University of Akron

RONALD H. SPECTOR. *Advice and Support: The Early Years, 1941-1960*. (United States Army in Vietnam.) Washington: Center of Military History. 1983. Pp. xviii, 391. Cloth \$18.00, paper \$11.00.

This first of a projected eighteen volumes inaugurates the U.S. Army's official history of the war in Vietnam.

Ronald H. Spector's book traces U.S. military involvement in Vietnam from World War II to

1960. The first part details the work of the various U.S. military missions to Vietnam during and immediately after the Second World War. A second section deals with American military assistance to France during the first Indochina war. The third and longest part analyzes the U.S. military advisory program for South Vietnam between 1954 and 1960.

Spector has done an excellent job. In terms of research, he has gone beyond the standards set by the official historians of World War II, examining numerous private manuscript collections and even British Foreign Office records and conducting extensive interviews in addition to exploiting the voluminous archives of the U.S. government and especially the army. The book is rich in new and interesting detail. Spector unravels the hitherto obscure history of the several World War II military missions in Indochina. He fills in the previously sketchy details about the military aid programs for France and South Vietnam. He clarifies the army's important role in the 1954 Dienbienphu crisis.

The Center for Military History has promised to be "even-handed" in its treatment of a controversial war, and Spector has lived up to that commitment. He is critical of the United States for persistently underestimating the strength of Vietnamese nationalism and of the army for repeatedly deluding itself into believing that military power alone could achieve American goals in Indochina. His critical analysis of the military training program in South Vietnam is especially valuable and timely. Explaining the various reasons why U.S. Army advisors failed to build an effective South Vietnamese Army, he concludes that the failure in the broadest sense underscored a Joint Chiefs of Staff warning of 1954 that "strong and stable governments and societies are necessary to support the creation of strong armies. That the reverse is seldom true," Spector goes on to say, "would be clearly and tragically demonstrated in the years to follow" (p. 379).

Although he sheds a good deal of light on early U.S. military involvement in Vietnam, Spector also goes over some ground that is familiar. It might therefore be questioned whether this official history would do better to concentrate on U.S. Army involvement and leave to others such things as the origins of the insurgency in South Vietnam.

This volume nevertheless marks an auspicious beginning for an important series. The research is impressive, the writing clear, and the analysis persuasive. The book is handsomely printed and illustrated. The Center for Military History is to be praised for publishing a book of this quality a mere nine years after the end of that most divisive war. Maintaining the objectivity of this projected series will, of course, become increasingly difficult as the volumes move into areas where the army's role was

more important and controversial. One can only hope that the center, with the unstinting support of the army, will continue to adhere to the standards originally set in the World War II histories and admirably upheld here.

GEORGE C. HERRING
University of Kentucky

FREDERICK C. MOSHER. *A Tale of Two Agencies: A Comparative Analysis of the General Accounting Office and the Office of Management and Budget*. (Miller Center Series on the American Presidency.) Baton Rouge: Louisiana State University Press. 1984. Pp. xxvi, 219. \$22.50.

Scholars of administrative history appreciate the difficulties in reaching meaningful conclusions about the federal bureaucracy. Frederick C. Mosher studies the General Accounting Office (GAO) and the Office of Management and Budget (OMB).

The comparisons abound. Both agencies were established in 1921 and fought periodically to resist partisan ideology. Each traced its origins to Progressivism. These agencies unsuccessfully resisted New Deal legislation. To this day they have displayed a fervor for conservatism, saving money, eliminating waste, and battling fraud. This frugality usually inhibited the agencies except during World War II, and scandals brewed undetected by either agency. The agencies today are scarcely larger than they were shortly after World War II. Despite notable exceptions, career workers in the agencies consistently reacted to events, finding fault with past governmental actions, present programs, or planned activities. They lost their monopolies: the GAO watched the rise of the Office of Technological Assessment and the Congressional Budget Office; the OMB lost certain powers to the Office of War Mobilization during World War II, the Council of Economic Advisers (1946), and the National Security Council (1947). These watchdogs lost their battle with the Department of Defense over continually increased funding.

Contrasts exist. Many GAO duties within the Treasury Department predated 1921. Experts usually considered the OMB more powerful than the GAO. Changes evolved more slowly at the GAO than in the OMB. The directors at the OMB generally served at the pleasure of the president; the GAO comptroller-general enjoyed a fifteen-year term. The OMB fought a routine set of deadlines, the GAO did not. Their audiences differed greatly, the GAO addressing Congress, often via television or the press. The OMB answered to the president, its importance often unknown to the public.

Collaboration seldom resulted. Organizational and social distance separated them, the OMB con-

sidered itself superior to the GAO. Rare examples of cooperation occurred during World War II and, since 1947, in joint accounting procedures. Two confrontations erupted. In the late 1950s the OMB attempted unsuccessfully to scrutinize the GAO budget. In 1959, the GAO undertook a comprehensive study of the Executive Office of the President including the OMB; it never gained access to the necessary data.

Mosher also summarizes activities and programs. The OMB (Bureau of the Budget until 1970) began as a business agent to the president, later introducing the performance and lump sum budgets, and assuming the role of advisor on pending congressional bills. It became unduly politicized beginning in the second Nixon administration.

The GAO plodded steadily onward against backlogs, often at odds with Treasury, valiantly checked U.S. programs during World War II, and prospered thereafter with commercial-type audits, effective management routine, and professionalized staff. It excelled particularly in reporting on Watergate and evaluating the poverty programs.

Mosher's readable book leans heavily on his own previous GAO expertise and that of Larry Berman's about the OMB. Its weaknesses are minor: repetition at times and occasionally a lack of specific examples to describe controversies with other agencies. This volume achieves significant comparative analysis and is a worthy addition to the Miller Center Series on the American Presidency.

DONALD R. WHITNAH
University of Northern Iowa

DAVID W. REINHARD. *The Republican Right since 1945*. Lexington: University Press of Kentucky, 1983. Pp. ix, 294. \$25.00.

The Republican Right since 1945 by David W. Reinhard traces GOP conservatism from the battle days of Ohio's "Mr. Republican," Robert A. Taft, when conservatives dominated the party's congressional leadership but failed in their repeated attempts to capture its presidential nomination, to 1964, when they captured the convention only to see their candidate, Barry Goldwater, overwhelmingly defeated. Two brief concluding chapters carry the story first through the Nixon years, when to the dismay of conservatives the new president embraced both Keynesian fiscal policies and the People's Republic of China, and then through the Republican renaissance of the late seventies. The book ends with Ronald Reagan's triumphant inauguration.

Reinhard's study focuses on the several hundred or so politicians and editorial writers who have since 1945 comprised the party's conservative leadership, and he recounts an almost endless succession of

bitter factional battles over nominating conventions, platforms, election strategies, legislation, and party leadership. Based on extensive research in both secondary sources and archives, it is packed with anecdotal details and pungent quotations, from Democratic leader Sam Rayburn's apt description of the 1946 congressional campaign as "a damn beef-steak election," to the *Washington Post's* (apparently serious) praise for the "special sensitivity and class" displayed by President-elect Ronald Reagan when he visited Washington in November 1980. It is generally accurate and those errors it does contain are for the most part minor, such as confusing Christine Jorgenson with Christine Keeler. It is not particularly well written, though, and is filled with almost every cliché of modern political journalism.

The book's principal failing, however, is intellectual, not literary. In the preface, Reinhard describes his work as "a necessarily selective, old-fashioned political narrative that seeks to explain larger social, economic, and political forces and trends by examining the ideas and activities of some of the major political actors." What we get, though, is not so much old-fashioned as it is superficial; not so much a narrative as a chronicle. And certainly there is no explanation of "larger social, economic, and political forces and trends." Instead we encounter all of the sound and fury of partisan strife, but learn all too little of its significance. It is as though Charles Beard and others like him had never lived or thought or struggled to make sense out of our past. There is no effort to locate Republican conservatism in an economic (or for that matter geographic, social, cultural, or intellectual) context. Did, for example, the Republican conservatives draw their support from the smaller, more entrepreneurial sector of American business? From firms producing for national, rather than international, markets? And if so how did this influence their political struggles with the more moderate wing of the party, which they often identified with eastern financial capital? How does one make sense of the shift among Republican conservatives from the antimilitarist, antistatist, isolationist impulses of the Taft years to the embrace of the state, in at least its military aspect, by Goldwater and Reagan? How does one explain the geographical shift from the Midwest to the South and Southwest? Or the curious combination of cultural anti-modernism with an emphasis on economic growth and technological innovation?

It is not just that these and other similar questions are not answered, but they are not even asked. We are left instead with a seemingly disembodied politics in which action proceeds without cause or consequence. And as a result we learn but little about our recent past.

ROBERT GRIFFITH
University of Massachusetts

JONATHAN MARTIN KOLKEY. *The New Right, 1960–1968, with Epilogue, 1969–1980*. Washington: University Press of America. 1983. Pp. xi, 403. Cloth \$26.75, paper \$15.50.

This is an attempt to describe the origins and development of a complex, interesting, and extremely important political and cultural movement. The New Right has been a major factor in each of the presidential elections since 1964, and one of its heroes, of course, triumphed in 1980. Historians seem virtually unable to write about modern American conservatism without sniggering, and thus there is a genuine need for thorough, objective scholarship in the field. Unfortunately, this study, a UCLA doctoral dissertation, adds little to our knowledge of anything.

Analysis is not among the author's strengths. He fails to provide us with a convincing definition of his subject, and he chooses to lump together George Lincoln Rockwell, Robert Welch, Gerald L. K. Smith, George Wallace, and Billy James Hargis with Robert Taft, William F. Buckley, Jr., Frank S. Meyer, Barry Goldwater, and James Burnham. All are said to be ignorant, racist, greedy, reactionary, and so on. All are made to appear guilty of each other's excesses. Even a violent mob, at one point, is labeled "New Rightist."

Sweeping generalities abound. We are told, for example, that the New Right denounced the fluoridation of public drinking water and "rejected the Civil Rights Movement because racial integration inevitably led to racial mongrelization." We are assured that "A pathetic simplicity runs through most New Right minds." (This from one who thinks that the decade of the sixties "was, in some respects, the nation's Periclean Age.") The Roman Catholic Church, the author declares, "has played a central role in the European Reactionary tradition for centuries stretching back to the Inquisition."

More specific statements are sometimes equally unenlightening, such as when we learn that Barry Goldwater's "education at the Staunton Military Academy, his heroic service during the Second World War, and his two decade association with the United States Air Force Reserve has also left a definite military imprint upon his thinking."

There are a few bright spots. Coverage of several issues in the elections of 1964 and 1968 are competently done, as are chapters dealing with the collapse of liberalism in 1977 and the rise of neoconservatism. The author read a great deal of ultraconservative and conservative secondary literature, and the bibliography is useful.

Still, this book is likely to be forgotten quickly. It lacks rigorous thought, objectivity, and broad research. Not a single manuscript collection is cited, and only a handful of oral interviews appear in the

footnotes. Modern conservatism in the United States continues to await its first-rate historians.

THOMAS C. REEVES
University of Wisconsin

BYRON E. SHAFER. *Quiet Revolution: The Struggle for the Democratic Party and the Shaping of Post-Reform Politics*. New York: Russell Sage. 1983. Pp. ix, 618. \$29.95.

Quiet Revolution is a revealing account of the battle for reform waged within the Democratic party between 1968 and 1972. The book is probably longer than it needs to be, and at times the narrative gets bogged down in unnecessary detail. On balance, however, Byron E. Shafer's work is a fascinating study written in clear and generally sprightly prose and based on solid research in the "uncleaned" papers of the Commission on Party Structure and Delegate Selection, private papers of involved individuals, and interviews with over one hundred and twenty participants in the reform effort.

A political scientist by training, Shafer declares in his introduction that he wanted to study reform within the Democratic party as part of the larger concepts of "institutional change and the circulation of elites." He does so, but the real strength of his book is the narrative record it provides about the Democrats' efforts at reforming the process for selecting delegates.

Shafer begins his account with the creation in June 1968 of the Hughes commission (officially the Commission on the Democratic Selection of Presidential Nominees) by disgruntled McCarthy partisans in Connecticut who were frustrated by the fact that the Democratic state convention had named only nine McCarthy delegates of the forty-four who would represent Connecticut at the national convention. Convinced that McCarthy's strength in the state merited a larger proportion, his supporters decided to take the lead in establishing a neutral commission to study the party's rules for delegate selection. Harold E. Hughes, then governor of Iowa, agreed to chair a commission representing a broad spectrum of reform-minded and reasonably prestigious Democrats.

The Hughes commission played an important role in shaping recommendations adopted at the 1968 Democratic convention that led to overwhelming changes in the procedures used to choose the delegates for the 1972 convention. The surprising adoption of the minority report of the rules committee was particularly critical, since it specifically banned certain practices used previously to select delegates. Ironically, arguments over the Vietnam platform captured the news media's attention on the day the convention passed the minority report by a

slim margin. As a result, most news stories either treated its adoption routinely or ignored it altogether. Only the *New York Times* commented on the potential for significant change inherent in the report. The actual task of implementing the recommendations for changing the methods of picking delegates fell to the Commission on Party Structure and Delegate Selection, chaired by Senator George McGovern. The bulk of Shafer's book focuses on the McGovern commission and its work.

Shafer concludes that the impact of the McGovern commission on contemporary American politics was wide and deep. Reform ended the automatic claim on the nomination process previously enjoyed by the "regular" party and its traditionally strong blue-collar constituencies. With an open selection process, aspiring candidates could profitably woo the young, women, and blacks. Neither George McGovern nor Jimmy Carter, Shafer argues, could have been nominated under unreformed rules. And, he adds, with an unreformed process for Republican party delegate selection, Ronald Reagan would almost surely have failed to mount a serious challenge to Gerald Ford in 1976, which would have made Reagan's victory in 1980 considerably more doubtful.

JIM F. HEATH
Portland State University

CANADA

DALE GIBSON *et al.* *Attorney for the Frontier: Enos Stutsman*. Winnipeg: University of Manitoba Press. 1983. Pp. 180. \$18.95.

Enos Stutsman was an obscure nineteenth-century attorney who practiced law and real estate speculation, not very successfully, in a remote border area. He dabbled in politics and government, but did not leave his mark in either. He was speaker of the Dakota Territorial House of Representatives for a term. In 1868 he introduced a woman's suffrage bill but it failed to pass. Stutsman died in his forties before he had time to accumulate much in the way of wealth and property. He did not keep a diary or write letters of any literary merit. This biography would probably not have been written had Stutsman not been the first qualified lawyer to plead a case on the Canadian prairies and had he not played a minor role in the Red River uprising of 1869-70.

The chapters that deal with these two episodes in Stutsman's life are the least significant parts of the book. The McLean trial, in which Stutsman journeyed north of the border to defend an accused murderer makes for interesting reading, mainly because of the curious court system that the Hudson's Bay Company operated in the Red River settlement. The judge appears to have functioned

rather more like a European examining magistrate than the impartial arbiter familiar in common law courts. Stutsman's appearance for the defense reduced the court to a shambles and helped discredit an institution whose authority was already crumbling. The trial had little permanent impact on the legal history of western Canada, since the courts of the Hudson's Bay Company ceased to exist a few years later when the region became part of Canada.

A couple of years after the McLean trial Stutsman once again played a part in events north of the forty-ninth parallel. The inhabitants of the Red River settlement refused to accept a Canadian takeover of the territory without consultation. Stutsman attempted to steer the leader of the protest movement, Louis Riel, in the direction of requesting annexation to the United States. His efforts backfired completely. The book tells us little we did not already know about this incident.

Nevertheless, this biography provides a very useful insight into the life of a frontier lawyer-businessman of this period. Stutsman's haphazard legal training, his numerous unsuccessful business ventures, his efforts to obtain a little security through political patronage, and his frequent moves from town to town are part of a pattern of life about which we know little. The biography also has a great deal of human interest because in one important respect Stutsman was quite atypical. A congenital birth defect had left him legless. To overcome such a major handicap and live an independent life in the mid-nineteenth century required extraordinary courage and resourcefulness.

The book is very competently researched and well-written. There are numerous illustrations and some useful maps.

R. C. MACLEOD
University of Alberta

PATRICIA T. ROOKE and R. L. SCHNELL. *Discarding the Asylum: From Child Rescue to the Welfare State in English-Canada (1800-1950)*. Lanham, Md.: University Press of America. 1983. Pp. xii, 497. Cloth \$29.75, paper \$18.75.

Over the last two decades historians have both discovered children and tried to explain their changing position in Western society. In contrast to the extravagant theories laid out by some writers in the genre, in *Discarding the Asylum* Patricia T. Rooke and R. L. Schnell offer a modest framework to explain changes in childhood in one small part of the Western community since the end of the eighteenth century. Nevertheless, historians of childhood everywhere will find in Rooke and Schnell's thesis a powerful explanatory device. It seems, for example, to help organize much of what we know about changes in British and American childhood during these years. The "discovery" of childhood in

Western society, Rooke and Schnell argue, gave rise to what they call a concept, or an ideology, of childhood that came to define both our perceptions and our treatment of children. This ideology, they explain, was articulated through the four criteria of dependence, protection, segregation, and delayed responsibility. In their effort to protect children from physical and moral danger and to segregate them from pernicious aspects of adult life, those in the middle classes particularly, extended youthful dependency and thus delayed youngsters from assuming full responsibility for their lives.

Rooke and Schnell apply their intriguing framework to the history of child rescue in English-speaking Canada. They show how, after reformers applied the criteria to their own children, they extended them to "dependent" youngsters as well. In the nineteenth century Canadians created orphan asylums in which they segregated and protected certain needy children. In the twentieth century they came to use foster homes and scientifically based child care services with the effect that the notion of delayed responsibility eventually applied to all English-Canadian children.

Writers of such a bold academic venture deserved better treatment from their editors and publishers. Rooke and Schnell examined a rich array of sources and mined out of them much fascinating information about children in workhouses, orphanages, and in foster homes and under adoption. Caught up in the web of these riches and sure of their own grasp of their complex thesis—in this review stripped of all of its subtleties—the authors do not give us the help we need to see how the rich detail comes together into a fully developed case study to illustrate their explanatory framework. We need more help in following them as they move through both time and space. We need tables that show us how many children they are discussing. We need an index that gives us more help in finding particular parts of the story. We need a guide through the sources. Unfortunately, these failures on the part of the University Press of America prevent *Discarding the Asylum* from achieving its full potential. Nonetheless, Rooke and Schnell have made an important addition to the theory and practice of the history of childhood. They deserve a wide readership.

NEIL SUTHERLAND
University of British Columbia

LATIN AMERICA

FRANCISCO CUEVAS CANCINO. *Bolívar en el tiempo*. 2d. ed. Mexico City: El Colegio de México. 1982. Pp. 463.

This work is not so much a biography of Simón Bolívar (1783–1830) as it is an interpretation of his actions and historical milieu. Structurally, the work

consists of seven chapters. The first is a lengthy commentary on the effects of Bourbon imperial policies; the remaining six concentrate on the career of Bolívar before 1822. *Bolívar en el tiempo* also includes the texts of thirteen Bolivarian documents, such as the famous letter from Jamaica (1815) and his address to the Congress of Angostura (1819); a brief bibliographical essay; a chronology; and indexes of people and places. Yet this is a difficult book for the beginner. Although the last six chapters are essentially chronological, they presume a familiarity with the basic facts of Bolívar's life.

Stimulating interpretations abound in the work. Francisco Cuevas Cancino criticizes the Bourbon family pact as one that consistently worked to the advantage of France and the disadvantage of Spain. He stresses the contradictory tendencies of Bourbon reforms, which encouraged progress and the rise of Creole consciousness in the Spanish empire at the same time that they stifled colonial achievements in order to centralize the benefits derived in the hands of the monarchs. Bolívar is described as a supremely talented military strategist: coldly calculating, deliberately sacrificing the immediate gain to win the long-term goal, and brilliantly correct in his choices. He emerges here as an authentic democrat whose actions led to real revolutionary changes within his centralist and often authoritarian framework, while more ideologically radical leaders actually contributed to a restoration of the Spanish aristocratic social system through well-intended but often ill-conceived legislation.

Cancino—Mexican lawyer, diplomat, author, historian—undoubtedly respects and admires Bolívar. One of the major achievements of this work is his success in conveying the epic nature of Bolívar's struggle for independence in Venezuela and Colombia. That is both the strength and the weakness of this work. The author makes frequent allusions to Homer's *Iliad* and *Odyssey*, and, indeed, trying to know and understand the struggle for independence from this work is often more analogous to reading Homer than Thucydides. For the critical scholar the author's tendency to take issue with the interpretations of others will be both exciting and frustrating. Even when some effort is made to explain the reasons in the text itself, the fact that the book is virtually without scholarly citations leaves the reader completely at loose ends. In the final analysis, this work is both well written and recommended reading, but the reader should not expect carefully documented research, a critical picture of Bolívar's actions, nor a definitive account of events. This is, rather, a provocative series of interpretive insights.

ROBERT H. DAVIS
Luther College

ENRIQUE D. DUSSEL. *Historia general de la Iglesia en América latina*. Volume 1, part 1, *Introducción general*

a la historia de la Iglesia en America latina. Salamanca: Sígueme. 1983. Pp. 723.

Liberation theology and its devotees in Latin America have sparked new interest, new methodologies, and new interpretations of the Roman Catholic Church in Latin America. Prime mover in the new historiography of the Latin American church is Enrique D. Dussel, editor of the projected eleven-volume *Historia general de la Iglesia en America latina*, which will be published by the Commission of Studies for Latin American Church History. Volume 1 is Dussel's general introduction, which sets the stage, thematic and methodological, for the entire series. Lengthy and ponderous chapters deal with Dussel's views on how church history as well as the European and preconquest social and religious histories of the hemisphere are to be rewritten. The colonial Latin American church, the "spiritual conquest," the diocesan organization of the church, its policy toward the native populations of America, and the various levels of religiosity of the colonial church (for example, formal religion, folk religion, religious syncretism) are examined in some detail. Here Dussel uses material from many of his other works and continues to mold the history into his theme of "the liberation of the poor." The chapter on "Concilios, cleros y religiosos" is an interesting survey of sixteenth- and seventeenth-century developments. His presentation of the daily social life of Christian peoples paints vibrant, if subjective, pictures of mass culture. The final chapter on the church and the Enlightenment of the eighteenth century examines, among other things, the crisis brought on by the philosophical eclecticism of the clergy. Dussel's training in the Germanic tradition leads him to document his ideas excessively, and his desire to be modern leads him to quantify, in all manner of charts, the colonial religious experience.

If one accepts Dussel's ideological premises one can agree with his arguments, but it does seem that his liberal and dependency stances often lead him to interpretations and conclusions that may be questioned. Actually, there is little that is new in this first volume, given that it is a synthesis of printed works rather than an attempt to view the church from new archival sources. Perhaps as an introductory volume to the entire series this approach was inevitable. The seven hundred pages of text are difficult reading and portend that this work is clearly for a specialized audience of scholars. No one can doubt, however, that the entire series is a monumental undertaking that will furnish an alternative series of perspectives to the understanding of the Latin American church.

RICHARD E. GREENLEAF
Tulane University

DAVID CARRASCO. *Quetzalcoatl and the Irony of Empire: Myths and Prophecies in the Aztec Tradition*. Chicago:

University of Chicago Press. 1982. Pp. xii, 233. \$20.00.

In David Carrasco's view, Mesoamerica's Quetzalcoatl myth cycle should be understood as a highly elaborated paradigmatic statement that "integrated and was used to interpret the multitude of social processes that constituted the urban tradition" from the time of Teotihuacan's empire (A.D. 250) until the last days of the Aztec empire (A.D. 1521). The name Quetzalcoatl itself means quetzal-plumed serpent. Paintings and relief sculpture at Teotihuacan, and many other archaeological sites, place a plumed serpent god at or near the center of monumental religious complexes, or in palace-like structures occupied by highly ranked politico-religious elites. Both the god and several influential priest-kings bearing the same name were celebrated in narrative and annalistic texts written down with the use of European script in the early years of Spanish colonial rule.

Carrasco, a historian of religions, provides detailed background accounts of pertinent archaeological and ethnohistorical source materials relating to six important Mesoamerican cities: Teotihuacan, Xochicalco, Tula, Chichén Itzá, Cholula, and Tenochtitlan. The first two are known almost exclusively from the archaeological record. For the last four, archaeological data may be combined with information derived from pictorial-glyphic manuscripts and a wide range of early colonial literary texts.

Although the author tells us little that is strikingly new about Quetzalcoatl as either deity or priest-king, his analysis breaks new ground in suggesting that iconography and historical fact had been ingeniously combined by pre-Spanish elites so that they presented "a paradigm of primordial order" actively involved in the birth, growth, stabilization, and even the sometimes abrupt dissolution of large and strongly centralized politico-religious centers (p. 65).

For the Aztecs of Tenochtitlan, Quetzalcoatl's role as patron of Tollan (modern Tula, Hidalgo), capital city of the Toltec empire, was inextricably involved with that of his priestly counterpart, who was founder, organizer, and ruler of this archetypal city. Even more, the same priest-king-god combination presided throughout the catastrophic end of Toltec dominion, which was manifested in loss of royal authority, the breaking of kingly honor, and the fall of an entire kingdom. Thus, the Quetzalcoatl tradition, as Carrasco persuasively observes, also exemplified "the weakness inherent in Toltec sovereignty and which also functioned as a model for the abdication of kingship" (p. 175).

Since Tenochtitlan's imperial status in the fifteenth and early sixteenth centuries was legitimized by a claimed descent of its kings from the lineage of Quetzalcoatl, the same paradigm not only served as the source for priestly conduct, law, and religious

custom at the Aztec capital but also ironically provided an unexpectedly precise model for Moctezuma's abdication of sovereign power while imprisoned by Cortés, as well as for the collapse of his empire in the course of the next several months.

There are, of course, formidable difficulties involved in the testing of this or any similarly conceived thesis relating religious ideologies to social and historical process. Although many of Carrasco's specific interpretations will no doubt be substantially revised in the light of future investigation, the fact remains that he has significantly advanced our understanding of the complexities of Mesoamerican culture history, not least by demonstrating that there was a great deal more involved in religious belief than its purely iconic context.

EDWARD E. CALNEK
University of Rochester

WOODROW BORAH. *Justice by Insurance: The General Indian Court of Colonial Mexico and the Legal Aides of the Half-Real*. Berkeley and Los Angeles: University of California Press. 1983. Pp. xviii, 479. \$45.00.

This latest of Woodrow Borah's many publications is the result of several decades of research, some of them in collaboration with Lesley Byrd Simpson, to whom the book is dedicated.

Borah's main purpose is to study the special court set up during the Spanish colonial period to deal with the legal affairs of the Indians of central Mexico. Successive chapters discuss the court's establishment; the kinds of cases brought before it and how they were resolved; the procedures, personnel, and work patterns of the court; how it was financed; and how it ended in the early nineteenth century. The picture is sharpened by a chapter that compares the court to similar systems in Yucatan and the Marquesado del Valle (Oaxaca), and to the audiencia of Guadalajara, perhaps somewhat typical of nonviceregal audiencia districts, which did not have an Indian court or legal insurance system. Borah also compares the court in New Spain to that of Peru, the other great viceroyalty, finding the Mexican system in general to have been more efficient, effective, and humane.

To thus survey the work, book review fashion, is to fall short of a true assessment, for, as one would expect from this author, it is much more broadly conceived and is far more than the sum of its parts. The court's historical origins were complex. How did Mediterranean rulers, especially those of Castille, deal with the problem of the poverty-stricken and their limited access to the law and to the ears of the powerful? These governments believed in, indeed were based on, a system of immutable hierarchies of classes and corporations. But these rulers, because of religious considerations and ones of self-

esteem and legitimacy, also wished to present themselves as paternalistically benevolent. To resolve this apparent paradox Mediterranean governments came to believe that the poor should have special access to quick, inexpensive, summary proceedings from the Roman *Ius Gentium* to the Castillian law codes.

In Iberia the emerging Christian states were faced with an additional complication. They contained large Jewish and, as the reconquest advanced, Islamic minorities. Then, as Iberia and Europe expanded overseas, they encountered even stranger peoples who shared nothing of the common European heritage, a new "wretched of the earth," but alien and often recently conquered. How these peoples were to be legally incorporated was a central question for European colonialism.

In the Caribbean islands the debate over this difficulty hardly began. The islanders were destroyed and the conquest passed on. But once on the Mexican mainland a concerned crown, sorting out belief in natural law, Christianity, special laws for the wretched, the conquest of the Canaries, its belief in hierarchies, and its concern for the spiritual and material welfare of its new and numerous Indian subjects, began to urge on its colonial administrators, themselves concerned that the Indian population was declining catastrophically and hoping not to repeat the disaster of the islands, a new system that would encompass all these concerns. The result was a special court for the Indians, headed by the viceroy and staffed by his salaried agents, where Indians, normally without paying fees, would have access to expedient, summary justice. The system was supported by a royal trust funded by a levy on Indian tribute payers, and thus served as a form of group legal insurance. The Indians quickly adapted to the system, using it often to bring some kind of out-of-court settlement from those above them. Some learned to use the court as one of many tools to bring pressure on other Indians, towns, groups, and colonial administrators. Borah finds that the court, solomonic rather than judgmental in its decisions, lessened exploitation and brought, if not equity—which it never intended—at least relief to many Indians. In all of this Borah's previous demographic work lends support to his research, especially in the pages that deal with the fund and its seventeenth-century difficulties in meeting the expenses of the court and its caseload. Anthropologically speaking, the court did not rescue Indian cultures as had been ambivalently intended. That game was over before the court began.

The author began the work with a large historical and philosophical context. He leaves us with a large historical and moral one. By creating one rule of law, governmental rationality, and coherent nation-states, the age of Enlightenment—and perhaps even more nineteenth-century liberalism—swept away

corporate anachronisms such as the Indian court. But did Mexico's peasants benefit from this new full citizenship? The abandonment of paternalism in caste-ridden societies may bring additional suffering to the wretched of the earth.

Boldly and intricately written, framed in large and troubling historical questions, yet thoroughly grounded in a specific historical situation, this volume is one of the most challenging and imaginative to appear in Latin American history in recent years.

MURDO J. MACLEOD
University of Arizona

EKKEHART KEEDING. *Das Zeitalter der Aufklärung in der Provinz Quito*. (Lateinamerikanische Forschungen: Beihefte zum Jahrbuch für Geschichte von Staat, Wirtschaft, und Gesellschaft Lateinamerikas, number 12.) Cologne: Böhlau. 1983. Pp. xiii, 591. DM 118.

This outstanding book on the Enlightenment in the historic Province of Quito covers the period from the mid-eighteenth century to the events of 1809–12 with its two revolutionary juntas; geographically it includes Popayán and Pasto in the north and Tomepanda and Borja in the south.

Ekkehart Keeding starts with a general discussion of the Hispanic Enlightenment and its many interpretations. As he states, his is a novel approach in that his research takes into account all the available evidence of the higher institutions of learning of a provincial Latin American capital (pp. 36–37). Thus, in the first part of the book he deals with the impact of the Enlightenment on the various religious orders, taking up institutions and libraries, literary and philosophic studies. He divides the religious orders into traditional, eclectic, and modern thinkers, and beyond simply analyzing the different scientific branches, he also refers to related problems (authority and experiment, the body-soul question, resistance, and popular sovereignty).

The center portion of his work delves into the secular world and includes literary criticism (the influence of Feijóo, humanism and Jansenism, rationalism and sensualism, the philosophes and the Inquisition, the private libraries of bishops and lay persons), the university with its *planes de estudios* between 1787 and 1803, and the natural sciences (geography with the famous Pedro Vicente Maldonado and the polemic around Francisco José de Caldas; the impact of the School of Leiden and the polemic around Eugenio de Santa Cruz y Espejo in medicine and anthropology, and botany with the scientific expeditions of 1782–1805). "Civilization" takes up the various projects to remedy Quito's seclusion, the Economic Society, and the printing press. Finally, "Revolution" analyzes, through sermons and pamphlets, the events of 1809–12, espe-

cially the rise of nationalism—which the author sees as the key problem—and the role of the natural law theories and the university.

What emerges is a well-researched study on the Enlightenment in Quito, the Quito version of the Hispanic or Christian Enlightenment, which, as in other parts of the Hispanic world, attempted to harmonize tradition with progress and faith with reason. The author concludes that in the period under study Quito uses the Enlightenment to emancipate itself from traditional authority, including—in the realm of philosophy—from Aristotelianism (with the exception of the Dominicans). Despite the strong inroads of the Enlightenment, the author argues "that the essential positions of the old Spanish, Scholastic and modern legal principles of public law, civil law and natural law were more or less known among the intellectuals, theologians, jurists, philosophy teachers and their students" (p. 520). Also, on the final page he states, and this reviewer wholeheartedly concurs, that "Enlightened Quito lacked religious criticism" and that "the lack of a Quitoan Jacobinism—with the exception of the pamphlets of 1795—clearly demonstrates that the basic social and political tenets of the reformers were rooted in Christian-Spanish culture and society" (p. 539).

It is a pity that the author did not include in his study such literature as that of Rafael Gómez Hoyos, Julio Halperin Donghi, Gregorio Maraón, Walter Hanisch Espíndola, Jaime Eyzaguirre, and Héctor José Tanzi's *Orígenes ideológicos del movimiento emancipador americano*, as well as my own *The Scholastic Roots of the Spanish-American Revolution*.

O. CARLOS STOETZER
Fordham University

NICHOLAS P. CUSHNER. *Jesuit Ranches and the Agrarian Development of Colonial Argentina, 1650–1767*. Albany: State University of New York Press. 1983. Pp. xi, 206. Cloth \$44.50, paper \$14.95.

With this volume, Nicholas P. Cushner has completed his trilogy on Jesuit economic activity in South America. To my mind, it is the most useful and satisfying of the three volumes. In addition to providing an excellent monographic analysis of the Jesuit contributions to the economy of Argentina, the author also offers a series of reflections on the collective character of Jesuit economic enterprise that are, in essence, the state of the art on the subject.

The present volume covers many of the same subjects as the previous two, including: a consideration of the physical setting and the acquisition of property by the order; the spacial development and organization of its enterprises; the operation of the haciendas; the labor systems; financing and profits;

and, finally, credit and trade. The book ends with a general conclusion for the three volumes.

The Jesuits organized activities around the mule trade with vineyards and cattle raising a distant second in importance. To guarantee the success of their enterprises, they used the system of colleges as a commercial network, manipulated their international connections to obtain credit, slaves, and supplies, and used their facilities to help those laymen who could help them. Cushner suggests that, in addition to their dedication to the task at hand, it was the commercial and credit facilities available to the Jesuits that provided the margin they enjoyed over their lay competitors (and also raised considerable envy among those competitors). They had adequate labor, supplies, and capital when others did not.

Where this volume diverges most noticeably from its two predecessors is in its consideration of the Jesuit aspect of these activities. Both previous volumes provided numerous details about the Jesuits, but the emphasis was on trying to understand rural enterprise rather than Jesuit behavior. Although not slighting this element in volume three, Cushner devotes considerable attention to examining how the Jesuits viewed their own activities and how they adapted, as an order, to the regional idiosyncracies of the economy of colonial Argentina.

In one of his most interesting chapters—on credit, money, and colonial trade—the author shows how the Jesuits' concern over economic health overcame both their own scruples and the vow of obedience to their superiors. The provincial leaders ignored repeated commands of the general in Rome to cease commercial dealings and excessive construction because of the scandals they caused. The provincials chose to ignore these commands because they judged that their activities were necessary for the economic well-being of the province.

Without being either moralistic or defensive, Cushner throughout the book shows considerable insight into the nature of the Jesuit order; the motivations, strengths, and weaknesses of the men who comprised it; and their interaction with the local community and environment. In so doing, the author has contributed significantly to both the economic and social history of the colonial period.

JAMES D. RILEY

Catholic University of America

DAVID BUSHNELL. *Reform and Reaction in the Platine Provinces, 1810–1852*. (University of Florida Monographs, Social Sciences, number 69.) Gainesville: University Presses of Florida. 1983. Pp. viii, 182. \$14.00.

The process of Argentine independence from Spain began in 1810. Throughout the first half of the

nineteenth century the influence of liberal ideas emanating from the European Enlightenment greatly influenced that process. These ideas not only contributed to the break from the mother country but also helped shape the form of the new national governments that eventually emerged from that effort.

David Bushnell's study is an attempt to trace these liberal influences in various provinces of the Rio de la Plata after independence. He concludes that although the victories of those who advocated liberal reform were neither consistent nor complete, they were substantial enough to mark the age and to withstand the powerful forces of reaction that sought to preserve the colonial status quo.

There is a considerable body of historical literature on both the details and broad outlines of this period in Argentine history. Bushnell, however, uses a new approach to examine in detailed quantitative fashion the specific acts of liberal and reform-oriented legislation (and the reaction to these measures) enacted by the various governments of the new Argentine nation. He focuses in particular on certain political changes, various social and economic innovations, and ecclesiastical reforms designed to limit the power of the Catholic church and provide greater religious freedom.

One of the main strengths of this effort is Bushnell's use of comparative perspective. On one level he examines reforms and counterreforms in six separate provinces and in Uruguay. His conclusion—that the province of Buenos Aires was the progressive forerunner of reform while the states of the interior were generally more conservative—produces few surprises. Nonetheless, the method used to test this proposition provides further hard evidence for the generally accepted interpretation and also suggests some of the more subtle provincial and regional differentiations that have marked Argentina's modern history. On another and perhaps more significant level, Bushnell compares the Argentine liberal experiment with that of the rest of Latin America. From this analysis he concludes that nowhere else in the region was liberal reform more widespread and profound than in the Platine provinces. The task for future historians is to determine just why this was so.

Perhaps the most important reinterpretation Bushnell provides deals with the rule of Juan Manuel de Rosas, the caudillo who dominated the new republic from 1825 to 1852. Widely viewed as the leading reactionary of his day, Bushnell's closer scrutiny of what actually occurred in the legislative and administrative realm shows that aside from political management "Rosas was a better liberal than either his admirers or his detractors generally admit" (p. 66).

In sum, Bushnell has dealt with some of the crucial questions of Argentina's early national his-

tory. Although the narrative is occasionally dry and repetitive, the work overall is clearly written, tightly argued, and based on meticulous research. The conclusions reached and the methodology employed should stimulate further investigation into these formative decades in the history of one of Latin America's most complex and intriguing societies.

RICHARD J. WALTER
Washington University

RICHARD W. SLATTA. *Gauchos and the Vanishing Frontier*. Lincoln: University of Nebraska Press. 1983. Pp. 271. \$21.95.

Romanticized as a folk hero and condemned as a barbarian, the gaucho has been generally misunderstood by both defenders and detractors since the late colonial period. In this well-researched monograph, Richard W. Slatta separates myth from reality in the varied and often tragic experiences of gauchos in the midst of the pampa's modernization.

The author summarizes in chapter 1 the diverse claims as to the origin of gauchos and of the word "gaucho." It is clear from the contents of the remaining chapters that the confusion and contradictions surrounding such relatively simple questions reflect the degree to which gauchos became pawns for people pursuing their own political and economic interests. In chapter 2, the author discusses briefly how the enormous extent of the humid pampa and the abundance of game, wild cattle, and horses provided for the emergence of the gaucho and his equestrian culture. The modernization of the pampas economy, however, brought a rapid end to this self-sufficient horseman.

These underlying messages, presented in the first two chapters, permeate the rest of the book: an inability or unwillingness to understand the gaucho in his own terms and the destruction of the ecological foundation for his life—open, frontier pampa. Changes in ranching reduced many gauchos to peons and seasonal workers, altered labor patterns, and rendered such prized skills as Creole horse-breaking obsolete (chap. 3). Discriminated against by both gauchos and ranchers, women had an even more difficult life (chap. 4). For both women and gauchos, European imports and influence caused major changes in clothing styles and recreational patterns (chap. 5).

The accumulation of extremely large landholdings, recourse to legal restrictions, forced military service, and the advent of fencing, sheep raising, and farming (chaps. 6–9) all served to bring an end to the gaucho. The elite had looked to Europe not only for economic models but also for immigrants to "civilize" the country. The latter became the object

of hatred by gauchos as representatives of everything bad that was happening to them (chap. 10). By the turn of the century the elite themselves began to reverse their perceptions. Immigrants were now seen as the destroyers not the saviors of the nation, and the gaucho was converted to a symbol of the true Argentina. By this time, however, the traditional gaucho had disappeared from the pampa (chap. 11).

Slatta's analysis is fascinating, but his emphasis on discriminatory elite behavior, though accurate, misdirects the reader away from a more fundamental proposition. Economic modernization inexorably doomed the gaucho. More enlightened land tenure policies, for example, would have benefitted immigrants—but not the gaucho, who scorned working on foot and tilling the land. Even ranching would have been too sedentary for the gaucho Slatta describes. Legal restrictions and elite practices were not the source of the gaucho's problem. They were more like bad frosting on an already unsavory cake. This minor shortcoming aside, Slatta's work makes a major contribution to our understanding of the impact of economic growth on rural Argentina while reminding us that development has a complex human dimension.

WILLIAM J. FLEMING
Pan American University

JOHN W. F. DULLES. *Brazilian Communism, 1935–1945: Repression during World Upheaval*. Austin: University of Texas Press. 1983. Pp. ix, 289. \$25.00.

The failure of the Brazilian Communist party (PCB) to mobilize the masses and challenge the status quo between 1935–45 is well known. It seems amazing that the PCB survived at all as it went through so many mutations and splits during these years. John W. F. Dulles's latest book on Brazil traces the fragmentation and disorganization of the party from the period that followed the bloody but unsuccessful November 1935 insurrection to August 1945, two months before the Vargas government fell.

Dulles presents an impressive display of names and events, both significant and trivial, that he gleaned from hundreds of interviews and from diligent review of innumerable newspapers, letters, and pamphlets. The resulting vast array of undigested raw material permits the English-reading public a glimpse of Brazilian reality, expands Robert J. Alexander's chapter on the Brazilian communists in *Communism in Latin America* (1957), and fleshes out Ronald Chilcote's *The Brazilian Communist Party* (1974).

Constant jailings and tortures of political prisoners are carefully spelled out; perhaps the book does

a better job of describing repression in Brazil than of describing what the communists were attempting. Mystery man and Comintern agent Arthur E. Ewart (also known as Harry Berger), who took Luís Carlos Prestes to Russia in 1931, was arrested in December 1935 and driven insane by the tortures inflicted by Brazilian police. The capture of Prestes on March 5, 1936, resulted in confusion and disarray within the PCB until his release from prison on April 18, 1945.

The Mario Pedroso, Trotskyite faction is traced, and the fascinating story of Herminio Sacchetta and the intraparty warfare is described. Dulles also reports the endeavors of the lawyer Sobral Pinto to defend Prestes, or at least make the Brazilian authorities follow the laws concerning prisoners.

When Brazil joined the war against Germany in August 1942, the Brazilian communists were forced to reassess their position regarding Vargas and the Estado Novo. Diogenes de Arruda Camara, from Bahia, attempted in August of 1943 to reorganize the Communist party at the Mantiqueira Conference. He set up the C.N.O.P. (provisional party national committee), which named Prestes as Secretary General even though he was in jail. Despite the presence of such veterans as Mauricio Grabois, João Amazonas, and Milton Caires de Brito, a large section of the Brazilian communists including Agildo Barata refused to accept the Mantiqueira decisions, because, according to them, the decisions did not represent the real PCB (PCB membership was estimated at less than 1000 at this time).

A small passage in the Dulles book catches the Byzantine political arrangements that often occur in Brazil. The Mexican foreign office attempted to obtain Prestes's freedom from prison so that he could live in Mexico with his relatives. The Brazilian ambassador was contacted by the Mexicans. But it turned out that the Brazilian ambassador, Carlos de Lima Cavalcanti, has once been accused of being a communist and enemy of the Estado Novo. He was now living in a "kind of exile" and refused to intervene.

As international events impinge so often on Brazilian internal affairs, it is surprising that no mention is made of the arrival in Brazil on February 17, 1945, of the U.S. Secretary of State, John Stettinius, directly from the Yalta Conference.

This is another informative volume by Dulles that will add to our understanding of why the Brazilian Communist party has been so ineffective.

JORDAN YOUNG
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HORTENSIA PICHARDO. *Biografía del Colegio de San Cristóbal de La Habana*. Havana: Academia de Ciencias de Cuba. 1979. Pp. 292.

Under the best of circumstances, education in colonial Cuba was always a problematical proposition—and circumstances were rarely the best. In the early nineteenth century there were fewer than 10,000 children enrolled in schools, out of a population of some 200,000 under fifteen years of age. There were only 7 schools in Havana, only 90 schools in all of Cuba. Some provincial centers were all but ignored. The province of Santiago de Cuba had a population of 328,000, and had only 991 children in school. The provincial capital of Santa Clara had only one school.

But the early nineteenth century was also a time of transition, and perhaps nowhere was the change overtaking Cuban society more evident than in the educational system. This time of transition serves as the setting for a remarkable study by Hortensia Pichardo, a member of the school of history in the faculty of humanities at the University of Havana. For the better part of the last five decades, Pichardo was an indefatigable researcher. Her interests embraced almost all of recorded Cuban history, from the first century of Spanish settlement to the most recent decades of Cuban socialism. Her study of the *colegio* of San Cristóbal in Havana is a work of immense erudition—meticulous in its construction, spacious in its conception, and provocative in its interpretation.

This is more than a "biography" of the *colegio* of San Cristóbal. It is a chronicle of an epoch spanning the last decade of the eighteenth century and the first quarter of the nineteenth. These were years of remarkable economic expansion. Sugar was transforming Cuba from a lethargic backwater colony into a bustling capitalist entrepôt, and the island would never be the same. Creole planters used their new economic power shrewdly, exacting from the court in Madrid political concessions, commercial privileges, and social parity. They also demanded better schools for their children. And Spain responded. During the early decades of the nineteenth century, the colonial government sponsored an expansion in the network of schools in Havana: the Escuela de Jesús (1806), Nuestra Señora de las Mercedes (1818), Academia Calasancia (1824), and the Colegio de San Fernando (1825). In 1827 the Academia Calasancia was reorganized and renamed Colegio de San Cristóbal.

Several factors combined to distinguish San Cristóbal from other *colegios*. It obtained generous financial support from the beginning and, as a result, enjoyed uncommon success in recruiting a first-rate faculty. Many instructors came from England, France, and Spain. The institution was also among the most liberal schools in Cuba. Indeed, most of the Spanish faculty members were political exiles from the peninsula seeking to escape the antiliberal wrath of Ferdinand VII. Some of Cuba's most original

thinkers of the nineteenth century were affiliated at one time or another with the Colegio de San Cristóbal. They include José de la Luz y Caballero, Francisco Ruiz, José Silverio Jorin, Felipe Poey, and José Fornaris. Not surprisingly, a generation of San Cristóbal students later became active participants in the nineteenth-century struggle for Cuban independence.

This is an important study, for it deals directly with the intellectual formation of a generation of Creoles who played an important part in shaping Cuban separatist thought. It is a model study, suggestive in its approach, definitive in its achievement. It is a most welcomed addition to the growing literature dealing with the nineteenth-century origins of Cuban nationality.

LOUIS A. PÉREZ, JR.
University of South Florida

LOUIS A. PÉREZ, JR. *Cuba between Empires, 1878–1902*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1983. Pp. xx, 490. \$32.95.

Louis A. Pérez, Jr. does a great service to students of Cuban history with this work: his book explains the political dimensions both of Spain's loss of sovereignty and of the U.S. hegemony that replaced it. He accomplishes this valuable task by placing at the center of his study a close examination of the Cuban independence movement. No one else writing in English has given us so complete and persuasive an understanding of the emotional strengths, political ambivalences, and organizational weaknesses of that movement.

Pérez's book is in the best tradition of the revisionist school of Cuban history. His work sustains several of that school's important themes: that the Cubans were on the verge of defeating Spain at the time of the U.S. intervention; that the aim of the intervention was to prevent Cuban independence; and that the life and works of José Martí represent the intellectual and emotional core of Cuban nationalism. In one important respect, however, Pérez differs with the revisionists. He demonstrates that U.S. domination was facilitated by the willingness of the different segments of the independence movement to accept or even to actively collaborate with U.S. policy and power. To explain this internal aspect of the failure of Cuban independence, Pérez examines the roots of the forces opposed to Spanish rule, tracing their multiple divisions along lines of class, sector, race, generation, organization, and ideology. Some of his most acute observations appear in his descriptions of upper-class Creole annexationists: North American-based exiles favoring a U.S. protectorate, other exiles like Martí opposing domina-

tion by both Spain and the U.S., civilian independence leaders in Cuba aspiring to a constitutional republic, and, finally, the officers of the independence army attracted by the idea of a populist-authoritarian state whose mass following added the potential for social revolution. The book follows the growing annexationist tendency among the exile leadership after the death of Martí and the intensifying struggle for direction of the rebellion on the island between the provisional government and the liberation army. The United States—despite its ignorance of Cuban politics, its preference for allies among conservative Cubans, and its barely concealed hostility toward Cuban independence—became the beneficiary of these divisions as each component of the anti-Spanish struggle competed for the favor of the new metropolitan power.

If the banner of Cuban independence is stained by these revelations, the Stars and Stripes hardly emerges immaculate. In a less fully documented, yet generally persuasive, segment of the book, Pérez describes U.S. policy as one of preparing the island not merely for U.S. predominance but for annexation. (In this regard he expands on the solid foundation laid twenty years ago by David Healy's *The United States in Cuba, 1898–1902*.) The author sees this goal as the outcome of a century-long assumption in the United States that the law of political gravitation would one day bring the island into the U.S. orbit. This notion was reinforced after 1898 by the belief among the occupation authorities that Cubans (except for what were termed the "better classes") were incapable of self-government.

Pérez is less successful in explaining why annexationism did not prevail. He outlines the steps by which the U.S. slowly and begrudgingly allowed Cuban independence (circumscribed, of course, by the Platt Amendment), but does not adequately explain why Washington settled for less than the annexation of which it appeared so confident. Perhaps this is because the book does not examine in-depth antiimperialist forces in the United States. Although giving the reader an excellent analysis of the forces comprising the movement for Cuban independence, Pérez has left for others the task of tracing the contradictory elements within the United States that caused her to seek both the liberation and domination of Cuba.

The above reservations aside, Pérez has answered so many important questions and has done so with such deft blending of perspectives, that we are deeply in his debt.

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WILLIAM W. BOYER. *America's Virgin Islands: A History of Human Rights and Wrongs*. Durham, N.C.: Caroli-

na Academic Press. 1983. Pp. xxiii, 418. Cloth \$27.75, paper \$13.75.

Most books on the U.S. Virgin Islands, following the euphoric character of the territory's tourist industry, tend to be flashy and melodramatic. William W. Boyer, however, has managed to write a serious history of this unincorporated tropical possession from the very beginnings, after 1492, up to the present time. Using both the extensive secondary literature and primary sources, he traces the social, political, and cultural development of the islands through the long Danish period up to the transfer in 1917, from the U.S. Navy administrative period up to 1934, and during the subsequent period of political and judicial reforms leading up to the present.

Boyer is sympathetic to the local reform movements in the islands. The perceived "wrongs" of his title derive from the fact that, despite locally elected legislative and executive branches, the territory is still governed by the arbitrary authority of the President and the Congress of the United States. As he notes in his final pages, the U.S. territorial clause continues to permit congressional and executive policies in Washington that are predicated on the assumption of absolute federal discretion with respect to U.S. territories. Therefore, the basic principles governing the federal-state relationship—equality of treatment and limitation of federal power—are not extended to the territories. The author thus concludes with a plea for a full-dress constitu-

tional reform that would finally terminate the Virgin Islands' second-class status and ensure that its citizens would at least possess the panoply of full self-government (pp. 377–78).

Implicitly at least, this is an argument for granting full incorporated statehood to the islands, as seems to be the majority electoral opinion in neighboring Puerto Rico. Curiously, the author does not examine other alternatives, such as national independence. He satisfies himself with the strange assertion—albeit relegated to a footnote—that "the pace of decolonization and the subsequent creation of new leftist mini-states in the Eastern Caribbean was gaining the increasing attention of Washington" (p. 371, n. 144). The only such left-wing island-state, in fact, was Grenada, whose socialist experiment finally disappeared with the grim events of October 1983. Boyer's further phrase about the dangers of "screaming for independence" (p. 371) perhaps indicates that, like most American liberals, he may be incapable of fully comprehending the vast Caribbean psychological revolution that over the last twenty years has been the basis of the anticolonial popular movements for national independence. The author's failure here, possibly, is related to his apparent unwillingness to discuss the Virgin Islands within the framework of the Caribbean regional system, taking pro-American sentiment in the Virgin Islands for granted. This is a grievous omission in an otherwise excellent book.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

GENERAL

JOHN BOSSY, editor. *Disputes and Settlements: Law and Human Relations in the West*. (Past and Present Publications.) New York: Cambridge University Press. 1983. Pp. ix, 296. \$49.50.

SIMON ROBERTS, The Study of Dispute: Anthropological Perspectives. EDWARD JAMES, *Beati pacifici*: Bishops and the Law in Sixth-Century Gaul. MICHAEL CLANCHY, Law and Love in the Middle Ages. DIANE OWEN HUGHES, Sumptuary Law and Social Relations in Renaissance Italy. JENNY WORMALD, The Blood Feud in Early Modern Scotland. RICHARD L. KAGAN, A Golden Age of Litigation: Castile, 1500–1700. J. A. SHARPE, "Such Disagreement betwix Neighbours": Litigation and Human Relations in Early Modern England. JAMES CASEY, Household Disputes and the Law in Early Modern Andalusia. NICOLE CASTAN, The Arbitration of Disputes under the *Ancien Régime*. JOHN R. GILLIS, Conjugal Settlements: Resort to Clandestine and Common Law Marriage in England and Wales, 1650–1850. JOHN BOSSY, Postscript.

THOMAS L. HASKELL, editor. *The Authority of Experts: Studies in History and Theory*. (Interdisciplinary Studies in History.) Bloomington: Indiana University Press. 1984. Pp. xxxix, 278. \$22.50.

THOMAS L. HASKELL, Introduction. ELLIOT FREIDSON, Are Professions Necessary? MAGALI SARFATTI LARSON, The Production of Expertise and the Constitution of Expert Power. THOMAS BENDER, The Erosion of Public Culture: Cities, Discourses, and Professional Disciplines. PETER DOBKIN HALL, The Social Foundations of Professional Credibility: Linking the Medical Profession to Higher Education in Connecticut and Massachusetts, 1700–1830. DAVID A. HOLLINGER, Inquiry and Uplift: Late Nineteenth-Century American Academics and the Moral Efficacy of Scientific Practice. DOROTHY ROSS, American Social Science and the Idea of Progress. THOMAS L. HASKELL, Professionalism versus Capitalism: R. H. Tawney, Emile Durkheim, and C.

S. Peirce on the Disinterestedness of Professional Communities. STEPHEN P. STICH and RICHARD E. NISBETT, Expertise, Justification, and the Psychology of Inductive Reasoning. SANFORD LEVINSON, Law as Literature: Do Legal Texts have Authoritative Interpretations?

PRESTON KING, editor. *The History of Ideas: An Introduction to Method*. Totowa, N.J.: Barnes and Noble or Croom Helm, London. 1983. Pp. 334. \$28.50.

PRESTON KING, Thinking Past a Problem. MICHAEL OAKESHOTT, The Activity of Being an Historian. PRESTON KING, Michael Oakeshott and Historical Particularism. R. G. COLLINGWOOD, The Historical Logic of Question and Answer. LEO STRAUSS, On Collingwood's Philosophy of History. A. O. LOVEJOY, The Study of the History of Ideas. MAURICE MANDELBAUM, On Lovejoy's Historiography. LEO STRAUSS, Political Philosophy and History. JOHN G. GUNNELL, The Myth of the Tradition. QUENTIN SKINNER, Conventions and the Understanding of Speech Acts. PRESTON KING, The Theory of Context and the Case of Hobbes.

SHULA MARKS and PETER RICHARDSON, editors. *International Labour Migration: Historical Perspectives*. (Commonwealth Papers, number 24.) Hounslow, United Kingdom: Maurice Temple Smith, for the Institute of Commonwealth Studies, University of London. 1984. Pp. viii, 280. £22.50.

SHULA MARKS and PETER RICHARDSON, Introduction. DAVID SOUDEN, English Indentured Servants and the Trans-atlantic Colonial Economy. CHARLOTTE ERICKSON, Why Did Contract Labour not Work in the Nineteenth-Century United States? GILL BURKE, The Cornish Diaspora of the Nineteenth Century. HUGH TINKER, Into Servitude: Indian Labour in the Sugar Industry, 1833–1970. PIETER EMMER, The Importation of British Indians into Surinam, Dutch Guiana, 1873–1916. ADRIAN GRAVES, The Nature and Origins of Pacific Islands Labour Migration to Queensland, 1863–1906. MARTIN LEGASSICK and FRANCINE DE CLERCQ, Capitalism and Migrant Labour in Southern Africa: The Origins and Nature of the System. PETER RICHARDSON, Coolies, Peasants, and Proletarians: The Origins of Chinese Indentured Labour in South Africa, 1904–1907. DONALD DENOON, The Political Economy of Labour Migration to Settler Societies: Australasia, Southern Africa, and

Southern South America, between 1890 and 1914. COLIN NEWBURY, *The Imperial Workplace: Competitive and Coerced Labour Systems in New Zealand, Northern Nigeria, and Australian New Guinea*.

AKIO OKOCHI and TADAKATSU INOUE, editors. *Overseas Business Activities*. (Proceedings of the Fuji Conference, 1982; International Conference on Business History, number 9.) Tokyo: University of Tokyo Press. 1984 Pp. xiii, 296. Y 5,000.

TADAKATSU INOUE, A Comparison of the Emergence of Multinational Manufacturing by U.S., European, and Japanese Firms. YOSUKE KINUGASA, Japanese Firms' Foreign Direct Investment in the U.S.: The Case of Matsushita and Others. KEIICHIRO NAKAGAWA, Comments. KEN'ICHI YASUMURO, The Contribution of Sogo Shosha to the Multinationalization of Japanese Industrial Enterprises in Historical Perspective. MASARU UDAGAWA, Comments. HIDEKI YOSHIHARA, Multinational Growth of Japanese Manufacturing Enterprises in the Postwar Period. MATAO MIYAMOTO, Comments. GEOFFREY JONES, The Expansion of British Multinational Manufacturing, 1890–1939. MASAMI KITA, Comments. M. YOTARO YOSHINO, The Evolution of United States Multinational Enterprises. TERUSHI HARA, Comments. GEOFFREY BLAINEY, The History of Multinational Factories in Australia. TAKESHI YUZAWA, Comments. TIEN-YI YANG, Foreign Business Activities and the Chinese Response, 1842–1937. KEIJIRO ISHIKAWA, Comments. FRANÇOIS CARON, Foreign Investments and Technology Transfers: The Case of the French Chemical Industry in the 1950s and 1960s as Viewed by the Direction des Industries Chimiques. EISUKE DAITO, Comments. TADAKATSU INOUE, Summary of Concluding Discussion.

KAY SAUNDERS, editor. *Indentured Labour in the British Empire, 1834–1920*. London: Croom Helm, distributed by Biblio Distribution Center, Totowa, N.J. 1984. Pp. 327. \$38.00.

WILLIAM A. GREEN, The West Indies and the Indentured Labour Migration: The Jamaican Experience. ALAN H. ADAMSON, The Impact of Indentured Immigration on the Political Economy of British Guiana. MARIANNE D. RAMESAR, Indentured Labour in Trinidad, 1880–1917. M. D. NORTH-COOMBES, From Slavery to Indenture: Forced Labour in the Political Economy of Mauritius, 1834–1867. BRIJ V. LAL, Labouring Men and Nothing More: Some Problems of Indian Indenture in Fiji. RAVINDRA K. JAIN, South Indian Labour in Malaya, 1840–1920: Asylum Stability and Involution. RAYMOND EVANS, 'Kings' in Brass Crescents: Defining Aboriginal Labour Patterns in Colonial Queensland. KAY SAUNDERS, The Workers' Paradox: Indentured Labour in the Queensland Sugar Industry to 1920. PETER RICHARDSON, Chinese Indentured Labour in the Transvaal Gold Mining Industry, 1904–1910.

ANTHONY SUTCLIFFE, editor. *Metropolis, 1890–1940*. Chicago: University of Chicago Press. 1984. Pp. viii, 458. \$40.00.

ANTHONY SUTCLIFFE, Urbanization, Planning and the Giant City. PETER HALL, Metropolis 1890–1940: Challenges and Responses. ANDREW LEES, The Metropolis and the Intellectual. THEDA SHAPIRO, The Metropolis in the Visual Arts: Paris, Berlin, New York, 1890–1940. PETER KEATING, The Metropolis in Literature. ANTHONY SUTCLIFFE, The Metropolis in the Cinema. DAVID HAROLD COX and MICHAEL NASLAS, The Metropolis in Music. LARS OLOF LARSSON, Metropolis Architecture. PATRICIA L. GARSIDE, West End, East End: London, 1890–1940. NORMA EVENSON, Paris, 1890–1940. HORST MATZERATH, Berlin, 1890–1940. KENNETH T. JACKSON, The Capital of Capitalism: The New York Metropolitan Region, 1890–1940. R. A. FRENCH, Moscow, the Socialist Metropolis. JÜRGEN REULECKE, The Ruhr: Centralization versus Decentralization in a Region of Cities. SHUN-ICHI J. WATANABE, Metropolitanism as a Way of Life: The Case of Tokyo, 1868–1930. PETER HALL, Metropolis, 1940–1990.

STEPHEN WILSON, editor. *Saints and their Cults: Studies in Religious Sociology, Folklore, and History*. New York: Cambridge University Press. 1983. Pp. xii, 435. \$59.50.

STEPHEN WILSON, Introduction. ROBERT HERTZ, translated by STEPHEN WILSON, St. Besse: A Study of an Alpine Cult. EVELYNE PATLAGEAN, translated by JANE HODGKIN, Ancient Byzantine Hagiography and Social History. PATRICK GEARY, Humiliation of Saints. GABRIELLE M. SPIEGEL, The Cult of St. Denis and Capetian Kingship. MICHAEL GOODICH, The Politics of Canonization in the Thirteenth Century: Lay and Mendicant Saints. PIERRE DELOOZ, translated by JANE HODGKIN, Towards a Sociological Study of Canonized Sainthood in the Catholic Church. ALBERT SOBOUL, translated by JANE HODGKIN, Religious Feeling and Popular Cults During the French Revolution: "Patriot Saints" and Martyrs for Liberty. STEPHEN WILSON, Cults of Saints in the Churches of Central Paris. PIERRE SANCHIS, translated by JANE HODGKIN and STEPHEN WILSON, The Portuguese *romarias*. MARC GABORIEAU, translated by JANE HODGKIN and STEPHEN WILSON, The Cult of Saints among the Muslims of Nepal and Northern India.

ANCIENT

BRIAN CROKE and ALANNA M. EMMETT, editors. *History and Historians in Late Antiquity*. New York: Pergamon. 1983. Pp. ix, 182. \$26.40.

BRIAN CROKE and ALANNA M. EMMETT, Historiography in Late Antiquity: an Overview. E. A. JUDGE, Christian Innovation and its Contemporary Observers. JOHN MATTHEWS, Ammianus' Historical Evolution. ALANNA M. EMMETT, The Digressions in the Lost Books of Ammianus Marcellinus. N. J. AUSTIN, Autobiography and History: Some Later Roman Historians and their Veracity. R. F. NEWBOLD, Patterns of Communication and Movement in Ammianus and Gregory of Tours. KATHERINE ADSHEAD, Thucydides and Agathias. C. E. V. NIXON, Latin Panegyric in the Tetrarchic and Constantinian Period. G. MASLAKOV, The Roman Antiquarian Tradition in Late Antiquity. PHILIP ROUSSEAU, The

Exegete as Historian: Hilary of Poitiers' Commentary on Matthew. BRIAN CROKE, The Origins of the Christian World Chronicle. GARRY W. TROMPF, The Logic of Retribution in Eusebius of Caesarea. KEN GARDINER, Paul the Deacon and Secundus of Trento. JOHN MOORHEAD, The West and the Roman Past from Theoderic to Charlemagne. ROGER SCOTT, Epilogue: Old and New in Late Antique Historiography.

MEDIEVAL

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Compiled By MELVIN HEATH

The titles of articles in the *AHR* are printed in italics, and titles of books reviewed are in quotation marks. Books of collected essays are designated by (E). The reviewer of a book is designated by (R), the author of a letter for the communications section by (C). Subject headings appear in small capitals.

- Abadi, Jacob, "Britain's Withdrawal from the Middle East, 1947-1971: The Economic and Strategic Imperatives," 776
"Abandoned Children," by Fuchs, 1336
Abbot, W. W., *et al.*, editors, "The Papers of George Washington, Colonial Series," vol. 1, "1748-August 1755"; vol. 2, "August 1755-April 1756," 845
Abbott, Carl (R), 209
Abella, Irving (R), 557
Abella, Irving, and Harold Troper, "None is Too Many: Canada and the Jews of Europe, 1933-1948," 886
Aberdeen, George Hamilton-Gordon, 768
"Abraham Clark and the Quest for Equality in the Revolutionary Era, 1774-1794," by Bogin, 194
Abraham, David (R), 467
Abrams, Philip, "Historical Sociology," 1298
"Acciaio per l'industrializzazione," edited by Bonelli (E), 574
"L'Accordo italo-russo di Racconigi," by Donnini, 1357
Achenbaum, W. Andrew (R), 193
Acosta Rodríguez, Antonio, "La población de Luisiana Española, 1763-1803," 843
"Actas del II Coloquio de Historia Medieval Andaluza: Hacienda y comercio, 1981" (E), 1197
Adams, Henry, 219
Adams, Herbert Baxter, 1225
Adams, Richard N., "Paradoxical Harvest: Energy and Explanation in British History, 1870-1914," 772
Adams, W. Lindsay, and Eugene N. Borza, editors, "Philip II, Alexander the Great, and the Macedonian Heritage" (E), 243
"L'Âge d'or de la caricature anglaise," by Jouve, 770
"The Adaptable Nation," by Fritz *et al.* (E), 249
Adelson, Roger (R), 773
Adler, Selig (R), 220
"L'Administration des provinces pannoniennes sous le Bas-Empire romain," by Fitz, 745
"Advice and Support," by Spector, 1411
"Affairs of Party," by Baker, 200
Afferica, Joan (R), 486
"After Brezhnev," edited by Byrnes, 496
"After 150 Years," edited by Alexander and Embry (E), 252
"The Age of Elizabeth," by Palliser, 423
"Agrarian Elites and Italian Fascism," by Cardoza, 160
AGRICULTURE: Bergad, "Coffee and the Growth of Agrarian Capitalism in Nineteenth-Century Puerto Rico," 888; Frandsen, "Vang og tægt," 1093; Gallet, "La seigneurie bretonne, 1450-1680," 1332; Grigg, "The Dynamics of Agricultural Change," 1051; Hahn, "The Roots of Southern Populism," 854; Illeritskaia, "Agrarnyi vopros," 171; Jones, "History of Agriculture in Ohio to 1880," 1161; Koval'chenko *et al.*, "Sotsial'no-ekonomicheskii stroi pomeschchich'ego khoziaistva Evropeiskoi Rossii v epokhu kapitalizma," 170; Liebman, "California Farmland," 856; Lindley, "Haciendas and Economic Development," 1189; Linz, "Der Bund der Landwirte in der Ersten Tschechoslowakischen Republik," 167; McAlpin, "Subject to Famine," 1139; Merl, "Der Agrarmarkt und die Neue Ökonomische Politik," 494; Olai, "Storskiftet i Ekebyborna," 462; Sachs, "The Invisible Farmers," 548; Takaki, "Pau Hana," 857; Thomson, "The Little General and the Rousay Crofters," 448; Yaney, "The Urge to Mobilize," 1118
"Der Agrarmarkt und die Neue Ökonomische Politik," by Merl, 494
"Agrarnyi vopros," by Illeritskaia, 171
Ahlund, Nils, 1094
"Ajatuksen ja toiminnan tiet," edited by Ikonen *et al.* (E), 250
Akenson, Donald H. (R), 449
"An Alabama Newspaper Tradition," by Hollis, 876
"Alba," by Maltby, 786
"L'Albero dei Belloni," by Caracciolo, 477
"Albert de Mun," by Levillain, 455
Albert, Peter J., and Ronald Hoffman, editors, "Arms and Independence: The Military Character of the American Revolution," 1387
Albertoni, Ettore A., editor, "Studies on the Political Thought of Gaetano Mosca: The Theory of the Ruling Class and Its Development Abroad" (E), 248
Albisetti, James C., "Secondary School Reform in Imperial Germany," 144
Aldcroft, Derek H. (R), 1077
Alden, John R., "Stephen Sayre: American Revolutionary Adventurer," 1147
Alder, Douglas D. (R), 160
Alderman, Geoffrey, "The Jewish Community in British Politics," 440
Alexander III (Romanov), 169

- Alexander III (the Great), 243
 "Alexander III and the State Council," by Whelan, 169
 Alexander, Archibald, 202
 Alexander, G. M., "The Prelude to the Truman Doctrine: British Policy in Greece, 1944–1947," 479
 Alexander, J. T. (R), 168
 Alexander, James W., "Ranulf of Chester: A Relic of the Conquest," 1321
 Alexander, John K. (R), 1305
 Alexander, Thomas B. (R), 520
 Alexander, Thomas G. (R), 1181
 Alexander, Thomas G., and Jessie L. Embry, editors, "After 150 Years: The Latter-day Saints in Sesquicentennial Perspective" (E), 252
 "All Men Are Created Equal: Ideologies, Rêves, et Réalités" (E), 571
 "All That Is Native and Fine," by Whisnant, 1183
 Allen, James B. (R), 1393
 Allen, James Sloan, "The Romance of Commerce and Culture: Capitalism, Modernism, and the Chicago-Aspen Crusade for Cultural Reform," 1409
 Allen, John L. (R), 206
 "The Alliance Israélite Universelle and the Jewish Communities of Morocco," by Laskier, 1371
 Allinson, Gary D. (R), 187
 Allmand, C. T., "Lancastrian Normandy, 1415–1450: The History of a Medieval Occupation," 1324
 "Alphonse de Lamartine," by Fortescue, 1085
 Altenstein, Karl, 143
 Alter, Peter, "Wissenschaft, Staat, Mäzene: Anfänge moderner Wissenschaftspolitik in Grossbritannien, 1850–1920," 123
 "Alternatives," edited by Howe (E), 1431
 "Altersschicksal und Altersmoral," by Sprandel, 1060
 Altholz, Josef L. (R), 442
 Altick, Richard D. (R), 1327
 Altschuler, Glenn C., and Jan M. Saltzgeber, "Revivalism, Social Conscience, and Community in the Burned-Over District: The Trial of Rhoda Bement," 204
 Alvarez de Toledo, Fernando, 786
 Amann, Peter H. (R), 1085
 "Ambivalent Conspirators," by Rossbach, 520
 Amelang, James S., *Baristers and Judges in Early Modern Barcelona: The Rise of a Legal Elite, 1264–84*
 "America's Burke," by Pencak, 194
 "America's China Sojourn," by Head, 539
 "America's Virgin Islands," by Boyer, 1423
 "American Apathy," by Genizi, 1406
 "American Aristides," by Brown, 514
 "American Beauty," by Banner, 191
The American Conception of National Security and the Beginnings of the Cold War, 1945–48 (Forum), by Leffler, 346–81
 "American Diplomatic History," by Combs, 234
 AMERICAN INDIAN HISTORY: Din and Nasatir, "The Imperial Osages," 1149; Dobyns, "Their Number Become Thinned," 1380; Dunlay, "Wolves for the Blue Soldiers," 208; Edmunds, "The Shawnee Prophet," 848; Keller, "American Protestantism and United States Indian Policy, 1869–1882," 208; Kelly, "The Assault on Assimilation," 870; McLoughlin, "Cherokees and Missionaries, 1789–1839," 1390; Schrader, "The Indian Arts and Crafts Board," 871; Wilson, "Ohiyesa," 858
 "American Labor and Immigrant History, 1877–1920s," edited by Hoerder, 529
 "The American Law School and the Rise of Administrative Government," by Chase, 875
 "American Physics in Transition," by Moyer, 1163
 "American Protestantism and United States Indian Policy, 1869–1882," by Keller, 208
 "American Protestantism's Response to Germany's Jews and Refugees, 1933–1941," by Nawyn, 872
 "American Sea Power in the Old World," by Still, 517
 "The American Style of Foreign Policy," by Dallek, 541
 Ammon, Harry (R), 1152
 Anchor, Robert (R), 404; 791
 "The Ancient Greeks," by Fine, 1312
 "Ancient Woodland," by Rackham, 755
 Andaya, Leonard Y., "The Heritage of Arung Palakka: A History of South Sulawesi (Celebes) in the Seventeenth Century," 510
 Andersen, William D. (R), 461
 Anderson, Benedict, and Audrey Kahin, editors, "Interpreting Indonesian Politics: Thirteen Contributions to the Debate" (E), 251
 Anderson, Edgar, "Latvijas Vēsture, 1920–1940: Ārpolitika," 1358
 Anderson, Gerald D., "Fascists, Communists, and the National Government: Civil Liberties in Great Britain, 1931–1937," 447
 Anderson, Irvine H. (R), 550
 Anderson, Malcolm, editor, "Frontier Regions in Western Europe" (E), 244
 Anderson, R. D., "Education and Opportunity in Victorian Scotland: Schools and Universities," 1331
 Anderson, Robert M. (R), 861
 Anfimov, A. M., "Krest'ianskoe khoziaistvo evropeiskoi Rossii, 1881–1904," 811
 "Anfänge westdeutscher Sicherheitspolitik, 1945–1956," vol. 1, "Von der Kapitulation bis zum Plevén-Plan," by Foerster *et al.*, 472
 "Die angelsächsische Königsurkunde im 7. und 8. Jahrhundert," by Scharer, 1063
 "Anglo-American Landscapes," by Mulvey, 93
 "Anglo-Danish Relations and Germany, 1933–1945," by Seymour, 461
 "Anglo-Norman Studies, V," edited by Brown (E), 895
 "Der Angriff auf die Sowjetunion," vol. 4 of "Das Deutsche Reich und der Zweite Weltkrieg," edited by Boog *et al.*, 1353
 Anisimov, E. V., "Podatnaia reforma Petra I: Vvedenie podushnoi podati v Rossii, 1719–1728 gg.," 1117
 Ankersmit, F. R., "Narrative Logic: A Semantic Analysis of the Historian's Language," 1037
 Anna, Timothy E., "Spain and the Loss of America," 1090
 "Antebellum Politics in Tennessee," by Bergeron, 525
 ANTHROPOLOGY: Boon, "Other Tribes, Other Scribes," 410; Collier *et al.*, editors, "The Inca and Aztec States, 1400–1800," 559; Garthwaite, "Khans and Shahs," 1368; Khuri, "Tribe and State in Bahrain," 1368
 Apostolidès, Jean-Marie, "Le roi-machine: Spectacle et politique au temps de Louis XIV," 451
 "Appalachia and America," edited by Batteau (E), 578
 Applebaum, Wilbur (R), 118; 401
 "April 10," by Weisser, 430
 "The Arabic-Speaking Lands," vol. 2 of "Christians and Jews in the Ottoman Empire," edited by Braude and Lewis (E), 1199
 "Arbeiter in Hamburg," edited by Herzog *et al.* (E), 572
 Arbeitsgemeinschaft Volksgruppenfrage, "Kein einzig Volk von Brüdern: Studien zum Mehrheiten-/Minderheitenproblem am Beispiel Kärntens" (E), 247
 ARCHAEOLOGY AND NUMISMATICS: Hanfmann *et al.*,

- "Sardis from Prehistoric to Roman Times," 739;
Hodges and Whitehouse, "Mohammed, Charlemagne, and the Origins of Europe," 1061;
Troxell, "The Coinage of the Lycian League," 1313
"Archaeology of Urban America," edited by Dickens (E), 253
ARCHITECTURAL AND LANDSCAPE HISTORY: Bennett, "The Mathematical Science of Christopher Wren," 118; Dellheim, "The Face of the Past," 123; Etlin, "The Architecture of Death," 1335; Harries, "The Bavarian Rococo Church," 464; Muthesius, "The English Terraced House," 768; Ragon, "The Space of Death," 1307
"The Architecture of Death," by Etlin, 1335
"Archival Theory and Practice in the United States," by Berner, 837
Arellano, Jorge Eduardo, "Bibliographía general de Nicaragua," pt. 1, "1674–1900," 1190
Arellano, Jorge Eduardo, "Diccionario de las letras nicaragüenses," pt. 1, "Escritores de la época colonial y el siglo XIX," 1190
Arellano, Jorge Eduardo, "Panorama de la literatura nicaragüense," 1190
"Aristocrats and Servitors," by Crummey, 808
"Aristotle and the Renaissance," by Schmitt, 755
Arlacchi, Pino, "Mafia, Peasants, and Great Estates: Society in Traditional Calabria," 800
"Arming the Chinese," by Chan, 183
"Arms and Independence," edited by Hoffman and Albert, 1387
Armstrong, David, "Political Anatomy of the Body: Medical Knowledge in Britain in the Twentieth Century," 445
Armstrong, John A. (R), 410; 1044
Armstrong, William M. (R), 1398
"The Army and Civil Disorder," by Cooper, 528
Arnstein, Walter L. (R), 122
ART HISTORY: Gibson, "The Santa Fe and Taos Colonies," 536; Guilbaut, "How New York Stole the Idea of Modern Art," 1177; Johns, "Thomas Eakins," 1398; Jouve, "L'Âge d'or de la caricature anglaise," 770; Larson, "The Reluctant Patron," 552; Lippincott, "Selling Art in Georgian London," 1327; Poesch, "The Art of the Old South," 1382; Scheibler, "Griechische Töpferkunst," 1311; Weisberg, editor, "The European Realist Tradition," 94
"Art, Myth, and Ritual," by Chang, 1373
"The Art of the Old South," by Poesch, 1382
Ardau, Denise (C), 1220
"As Minority Becomes Majority," by Sealander, 547
Ascher, Abraham (R), 172
Ash, James L., Jr., "Protestantism and the American University: An Intellectual Biography of William Warren Sweet," 222
Asker, Björn, "Officerarna och det svenska samhället, 1650–1700," 1344
"The Assault on Assimilation," by Kelly, 870
Associazione Toscana della nobiltà Italiana *et al.*, "I ceti dirigenti in Toscana nell'età precomunale" (E), 248
Aston, T. H., *et al.*, editors, "Social Relations and Ideas: Essays in Honour of R. H. Hilton" (E), 241
"At the Brink of War and Peace," edited by Vucinich, 481
"At the Crossroads," edited by Kornberg, 1366
Athearn, Robert G., 578
"Athenian Popular Religion," by Mikalson, 1057
Atkinson, Clarissa W., "Mystic and Pilgrim: The Book and the World of Margery Kempe," 749
Atkinson, Dorothy (R), 811
Atkinson, Dorothy, "The End of the Russian Land Commune, 1905–1930," 1119
"Atlantic Empires," by Liss, 92
Atleson, James B., "Values and Assumptions in American Labor Law," 546
"Atti del simposio internazionale Cateriniano-Bernardiniano, Siena, 17–20 Aprile 1980," edited by Maffei and Nardi (E), 574
"Attorney for the Frontier," by Gibson *et al.*, 1415
Atwell, John (R), 488
Auerbach, Jerold S., "Justice without Law? Resolving Disputes without Lawyers," 533
"Aufklärung und Pietismus im dänischen Gesamtstaat, 1770–1820," edited by Lehmann and Lohmeier (E), 1199
"Augustan England," by Holmes, 119
Augustinos, Gerasimos (R), 162
"Aus Kirche und Reich," edited by Mordek (E), 1427
"Aussenpolitik und Öffentlichkeit in Russland, 1906–1912," by Ferenczi, 491
Austen, Ralph A. (R), 820
"Australia and Britain," edited by Madden and Morris-Jones (E), 252
"Austria since 1945," edited by Wright (E), 574
"Austromarksizam i jugoslavenko pitanje," by Redžić, 804
"Authoritarianism in Greece," by Kofas, 802
"The Authority of Experts," edited by Haskell (E), 1425
AUTOBIOGRAPHIES, DIARIES, AND MEMOIRS: Abbot *et al.*, editors, "The Papers of George Washington, Colonial Series," vol. 1, "1748–August 1755"; vol. 2, "August 1755–April 1756," 845; Hill, editor, "Die Weizsäcker-Papiere, 1900–1932," 150; Hoare, editor, "The *Resolution* Journal of Johann Reinhold Forster, 1772–1775," 120; Levenson *et al.*, editors, "The Letters of Henry Adams, 1858–1892," 219; Wachten, editor, "Theodor Herzl: Briefe und Tagebücher," vol. 1, "Theodor Herzl: Briefe und autoiographische Notizen, 1866–1895," 1111
"The Automobile and Urban Transit," by Barrett, 1172
Aveling, Marian (R), 1379
Avrich, Paul (R), 1121
"Awakening American Education to the World," by Byrnes, 544
Axelrod, Paul, "Scholars and Dollars: Politics, Economics, and the Universities of Ontario, 1945–1980," 237
Axtell, James (R), 839
Aymard, Maurice, editor, "Dutch Capitalism and World Capitalism," 789
Bacchi, Carol Lee, "Liberation Deferred? The Ideas of the English-Canadian Suffragists, 1877–1918," 236
Backer, John H., "Winds of History: The German Years of Lucius DuBignon Clay," 796
Bade, Klaus J., "Vom Auswanderungsland zum Einwanderungsland? Deutschland, 1880–1980," 795
Bagchi, Amiya Kumar (R), 829
Bagley, J. J., 1326
Bailey, Hugh C. (R), 1381
Baily, Charles M., "Faint Praise: American Tanks and Tank Destroyers during World War II," 869
Baird, Jay W. (R), 466
Baker, Jean H., "Affairs of Party: The Political Culture of Northern Democrats in the Mid-Nineteenth Century," 200

- Baker, Lewis, "The Percys of Mississippi: Politics and Literature in the New South," 1171
- Baker, Paula, *The Domestication of Politics: Women and American Political Society, 1780–1920*, 620–47
- Baker, Robert D., and Robert S. Maxwell, "Sawdust Empire: The Texas Lumber Industry, 1830–1940," 527
- Bakken, Gordon Morris, "The Development of Law on the Rocky Mountain Frontier: Civil Law and Society, 1850–1912," 855
- Balfour, Michael, "West Germany: A Contemporary History," 156
- Ball, Desmond, "Politics and Force Levels: The Strategic Missile Program of the Kennedy Administration," 554
- Ballard, Jack Stokes, "The Shock of Peace: Military and Economic Demobilization after World War II," 233
- Ballhatchet, Kenneth (R), 833
- "The Baltic States," by Misiunas and Taagepera, 807
- Banac, Ivo, and Paul Bushkovitch, editors, "The Nobility in Russia and Eastern Europe," 807
- Banac, Ivo, editor, "War and Society in East Central Europe," vol. 13, "The Effects of World War I: The Class War after the Great War; The Rise of Communist Parties in East Central Europe, 1918–1921" (E), 575
- "The Banana Wars," by Langley, 882
- Banner, Lois W. (R), 1393
- Banner, Lois W., "American Beauty," 191
- Banning, Lance (R), 1391
- "La Banque de France et ses deux cents actionnaires sous le Second Empire," by Plessis, 134
- "Barbarians and Romans," by Randers-Pehrson, 415
- Bargar, B. D. (R), 515
- Barker, Elisabeth, "The British between the Superpowers, 1945–1950," 1331
- Barkin, Kenneth (R), 147
- Barlow, Frank, "William Rufus," 1319
- Barnard, John, "Walter Reuther and the Rise of the Auto Workers," 227
- Barnes, Catherine A., "Journey from Jim Crow: The Desegregation of Southern Transit," 1411
- Barnhart, Donald S. (R), 1186
- Barr, David L., and Nicholas Piediscalzi, editors, "The Bible in American Education: From Source Book to Textbook" (E), 252
- Barratt, Glynn, "Russian Shadows on the British Northwest Coast of North America, 1810–1890: A Study of the Rejection of Defence Responsibilities," 1044
- Barrett, Paul, "The Automobile and Urban Transit: The Formation of Public Policy in Chicago, 1900–1930," 1172
- "Barrington Moore, Jr.," by Smith, 403
- Barristers and Judges in Early Modern Barcelona: The Rise of a Legal Élite*, by Amelang, 1264–84
- Bartlett, C. J. (R), 1044
- Bartley, Numan V., "The Creation of Modern Georgia," 1144
- Bartoli, Cosimo, 1108
- Barudio, Günter, "Gustav Adolf—der Grosse: Eine politische Biographie," 1095
- Basarab, John, "Pereiaslav 1654: A Historiographical Study," 808
- "Baseball's Great Experiment," by Tygiel, 877
- Baskerville, Peter A. (R), 237
- Bater, James H. (R), 170; 1119
- Bates, David, "Normandy before 1066," 113
- Batkay, William M. (R), 166
- Batteau, Allen, editor, "Appalachia and America: Autonomy and Regional Dependence" (E), 578
- Bauer, Arnold J. (R), 563; 1300
- Bauer, Franz J., "Flüchtlinge und Flüchtlingspolitik in Bayern, 1945–1950," 154
- Baugh, Daniel A. (R), 1328
- "The Bavarian Rococo Church," by Harries, 464
- Baxter, Maurice (R), 846
- Baxter, Stephen B. (R), 1092
- Bayat, Mangol (R), 1367
- Bayly, C. A., "Rulers, Townsmen, and Bazaars: North Indian Society in the Age of British Expansion, 1770–1870," 504
- Beale, Edward F., 852
- Beard, Geoffrey, "Craftsmen and Interior Decoration in England, 1660–1820," 427
- Beck, J. Murray, "Joseph Howe," vol. 1, "Conservative Reformer, 1804–1848," 235
- Becker, David G., "The New Bourgeoisie and the Limits of Dependency: Mining, Class, and Power in 'Revolutionary' Peru," 891
- Becker, William H., and Samuel F. Wells, Jr., editors, "Economics and World Power: An assessment of American Diplomacy since 1789" (E), 1430
- Beechert, Edward (R), 857
- Beer, Barrett L. (C), 588
- Beers, Burton F. (R), 884
- Beers, Clifford W., 548
- Beevor, Antony, "The Spanish Civil War," 1090
- "Before Color Prejudice," by Snowden, 103
- Beik, William (R), 1334
- Bell, Donald Howard (R), 160
- Bell, Rudolph M. (R), 800
- Bellamy, J. G. (R), 756
- "Belle Boyd," by Scarborough, 853
- Beluche, Renato, 887
- Bement, Rhoda, 204
- "Ben Tillett," by Schneer, 445
- Bender, Thomas (R), 1409
- Bendersky, Joseph W., "Carl Schmitt: Theorist for the Reich," 152
- "Benedictine Culture, 750–1050," edited by Lourdaux and Verhelst, 1318
- "The Benevolent Deity," by Wilson, 1385
- Benigno, Francesco, "Il porto di Trapani nel Settecento: Rotte, traffici, esportazioni, 1674–1800," 1354
- Benjamin, Jules R. (R), 1423
- Benko, Stephen (R), 1292
- Benner, Judith Ann, "Sul Ross: Soldier, Statesman, Educator," 1162
- Bennett, Edward M. (R), 174
- Bennett, Edward W. (R), 1352
- Bennett, J. A., "The Mathematical Science of Christopher Wren," 118
- Bennett, Michael J., "Community, Class, and Careerism: Cheshire and Lancashire Society in the Age of Sir Gawain and the Green Knight," 112
- Benson, John, "The Penny Capitalists: A Study of Nineteenth-Century Working-Class Entrepreneurs," 1075
- Benson, Robert L., et al., editors, "Renaissance and Renewal in the Twelfth Century," 109
- Bentley, Jerry H., "Humanists and Holy Writ: New Testament Scholarship in the Renaissance," 1039
- Bentley, Michael, and John Stevenson, editors, "High and Low Politics in Modern Britain: Ten Studies" (E), 896
- Benz, Wolfgang, and Hermann Graml, editors, "Europa nach dem Zweiten Weltkrieg, 1945–1982: Das Zwanzigste Jahrhundert II" (E), 1196
- Bergad, Laird W., "Coffee and the Growth of Agrarian Capitalism in Nineteenth-Century Puerto Rico," 888

- Berger, David, editor, "The Legacy of Jewish Migration: 1881 and Its Impact" (E), 895
- Berger, Martin (R), 1304
- Bergeron, Louis, *et al.*, "La statistique en France à l'époque napoléonienne" (E), 246
- Bergeron, Paul H., "Antebellum Politics in Tennessee," 525
- Bergman, Jay, "Vera Zasulich: A Biography," 814
- Berkin, Carol Ruth (R), 1147
- Berlatsky, Joel (R), 1325
- Berlin, Ira, and Ronald Hoffman, editors, "Slavery and Freedom in the Age of the American Revolution," 512
- Berman, Edward H., "The Influence of the Carnegie, Ford, and Rockefeller Foundations on American Foreign Policy: The Ideology of Philanthropy," 1184
- Berman, Harold J., "Law and Revolution: The Formation of the Western Legal Tradition," 1040
- Berman, Myron (R), 1179
- Berner, Richard C., "Archival Theory and Practice in the United States: A Historical Analysis," 837
- Berthold, Richard M. (R), 103
- Best, Geoffrey (R), 96
- Betten, Neil (R), 222
- "Between Orthodox and the Enlightenment," by Heyd, 474
- Beyer, Charles Jacques, "Nature et valeur dans la philosophie de Montesquieu: Analyse méthodique de la notion de rapport dans *L'Esprit des Lois*," 451
- Beyond Consensus: Richard Hofstadter and American Historiography*, by Singal, 976–1004
- "Beyond Sixty-Five," by Haber, 193
- "Beyond the Ballot Box," by Ryan, 523
- "Die Beziehungen Preussens zu Dänemark von 1688 bis 1789," by Hartmann, 1348
- Bezucha, Robert (R), 134
- "The Bible and American Arts and Letters," edited by Gunn (E), 1431
- "The Bible and Social Reform," edited by Sandeen (E), 252
- "The Bible in American Education," edited by Barr and Piediscalzi (E), 252
- "Bibliografía general de Nicaragua," pt. 1, "1674–1900," by Arellano, 1190
- BIBLIOGRAPHY: Arellano, "Bibliografía general de Nicaragua," pt. 1, "1674–1900," 1190; Arellano, "Diccionario de las letras nicaragüenses," pt. 1, "Escritores de la época colonial y el siglo XIX," 1190; Magocsi, "Galicia," 1115
- Biddle, James, 849
- Bidelman, Patrick Kay, "Pariahs Stand Up! The Founding of the Liberal Feminist Movement in France, 1858–1889," 136
- Biers, William R. (R), 1311
- Binion, Rudolph (R), 152
- "Biografía del Colegio de San Cristóbal de La Habana," by Pichardo, 1422
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- Hoover," 545; Neal, "Dark Horse," 1405; Niven, "Martin Van Buren," 199; Olien, "Morpeth," 432; Pacheco, editor, "The Legacy of George Mason," 1389; Packe, "King Edward III," 749; Page, "Perón," 893; Papanikolas, "Buried Unsung," 229; Perinbam, "Holy Violence," 1129; Peterson, "Littleton Waller Tazewell," 1394; Phillips, "Jedidiah Morse and New England Congregationalism," 515; Postma, "Viglius van Aytta als humanist en diplomaat, 1507–1549," 1343; Reagan, "H. I. Kimball, Entrepreneur," 526; Richard, "Saint Louis," 751; Richardson *et al.*, "Charles Willson Peale and His World," 847; Rothberg, "To Set a Standard of Workmanship and Compel Men to Conform to it": John Franklin Jameson as Editor of the American Historical Review, 957–75; Rowse, "Eminent Elizabethans," 424; Rubanowice, "Crisis in Consciousness," 791; Salmond, "A Southern Rebel," 224; Scarborough, "Belle Boyd," 853; Schneer, "Ben Tillett," 445; Shirley, "Thomas Harriot," 1069; Siepel, "Rebel," 852; Simms, "William Molyneux of Dublin, 1656–1698," 129; Singal, *Beyond Consensus: Richard Hofstadter and American Historiography*, 976–1004; Sivéry, "Saint Louis et son siècle," 1324; Smith, "Barrington Moore, Jr.," 403; Spiers, "Radical General," 1071; Sullivan, "Thomas-Robert Bugeaud," 133; Surratt, "Gottlieb Schober of Salem," 842; Symcox, "Victor Amadeus II," 1355; Tarling, "The Burthen, the Risk, and the Glory," 837; Taylor, "Laurence Oliphant, 1829–1888," 121; Thompson, "Edward F. Beale and the American West," 852; Urofsky, "A Voice That Spoke for Justice," 225; Vale, "Edward III and Chivalry," 749; Wallace-Hadrill, "Suetonius," 1314; Webb, "Tree Rings and Telescopes," 534; Weisbrot, "Father Divine and the Struggle for Racial Equality," 542; Wiener, "Radicalism and Freethought in Nineteenth-Century Britain," 432; Wilken, "John Chrysostom and the Jews," 1059; Wilson, "Ohiyesa," 858; Wood, *La Salle: Discovery of a Lost Explorer*, 294–323; Wooden, "John Foxe," 758; Wootton, "Paolo Sarpi," 1109; Yavetz, "Julius Caesar and His Public Image," 741
- BIOLOGICAL SCIENCES:** Bowler, "The Eclipse of Darwinism," 402; Browne, "The Secular Ark," 100; Rehbock, "The Philosophical Naturalists," 1072
- Birke, Adolf M., and Kurt Kluxen, editors, "Viktorianisches England in deutscher Perspektive" (E), 1198
- Birn, Donald S. (R), 1081
- Birn, Raymond (R), 779
- "The Birth of a New Europe," by Hamerow, 736
- "The Birth of Vietnam," by Taylor, 834
- "Der Bischof in merowingischer Zeit," by Scheibelreiter, 1317
- Bishko, C. J. (R), 1341
- Biskupski, Mieczyslaw B., and Stanislaus A. Blejwas, editors, "Pastor of the Poles: Polish-American Essays Presented to Right Reverend Monsignor John P. Wodarski in Honor of the Fiftieth Anniversary of His Ordination" (E), 253
- "Biudzhetnye obsledovaniia krest'ianskikh khoziaistv 20–kh godov kak istoricheskii istochnik," by Bokarev, 1361
- Björk, Ragnar, "Den historiska argumenteringen: Konstruktion, narration och kolligation—förklaringsresonemang hos Nils Ahnlund och Erik Lönnroth," 1094
- BLACK AND AFRICAN AMERICAN HISTORY:** Flynn, "White Land, Black Labor," 1161; Foner, "Nothing But Freedom," 1160; Harlan, "Booker T. Washington," 218; Howard, "Black Liberation in Kentucky," 1396; Johnson and Campbell, "Black Migration in America," 542; Joyce, "Gatekeepers of Black Culture," 211; MacDonald, "Blacks and White TV," 555; McCarthy, "Dark Continent," 1369; Meier and Rudwick, J. *Franklin Jameson, Carter G. Woodson, and the Foundations of Black Historiography* (Research Note), 1005–15; Naison, "Communists in Harlem during the Depression," 1174; Summerville, "Educating Black Doctors," 1166; Walker, "Free Frank," 1396; Weisbrot, "Father Divine and the Struggle for Racial Equality," 542
- "The Black Death," by Gottfried, 1322
- "Black Diamonds," by Schweider, 229
- Black, Ian, "A Gambling Style of Government: The Establishment of the Chartered Company's Rule in Sabah, 1878–1915," 509
- "Black Liberation in Kentucky," by Howard, 1396
- "Black Migration in America," by Johnson and Campbell, 542
- "The Black Press in the South, 1865–1979," edited by Suggs (E), 1202
- Blackett, R. J. M., "Building an Antislavery Wall: Black Americans in the Atlantic Abolitionist Movement, 1830–1860," 1159
- "Blacks and White TV," by MacDonald, 555
- "Blacks in Topeka, Kansas, 1865–1915," by Cox, 859
- Blake, John B. (R), 1049
- Blakeley, Barry B. (R), 1373
- Blejwas, Stanislaus A., and Mieczyslaw B. Biskupski, editors, "Pastor of the Poles: Polish-American Essays Presented to Right Reverend Monsignor John P. Wodarski in Honor of the Fiftieth Anniversary of His Ordination" (E), 253
- Blinov, A. S., "Tsentrāl'nyi sovet fabzavkomov Petrograda, 1917–1918 gg.," 817
- Blum, Jerome (R), 1293
- Blumenthal, Uta-Renate (R), 1317
- Blumenthal, Uta-Renate, editor, "Carolingian Essays," 752
- Blyth, John A., "English University Adult Education, 1908–1958: The Unique Tradition," 443
- Boardman, John, *et al.*, editors, "The Cambridge Ancient History," vol. 3, pt. 1, "The Prehistory of the Balkans; and the Middle East and the Aegean World, Tenth to Eighth Centuries B.C.," 1054
- Boari, Marco, "Qui venit contra iura: Il *furiosus* nella criminalistica dei secoli XV e XVI," 1105
- Bodenhamer, David J. (R), 1155
- Bodnar, John (R), 1164
- Bogdanor, Vernon, editor, "Liberal Party Politics" (E), 246
- Bogin, Ruth, "Abraham Clark and the Quest for Equality in the Revolutionary Era, 1774–1794," 194
- Bogue, Allan G. (R), 525
- Bogue, Allan G., "Clio and the Bitch Goddess: Quantification in American Political History" (E), 1202
- Bohstedt, John, "Riots and Community Politics in England and Wales, 1790–1810," 429
- Bokarev, Iu. P., "Biudzhetnye obsledovaniia krest'ianskikh khoziaistv 20–kh godov kak istoricheskii istochnik," 1361
- Boles, John B. (R), 196; 1384
- Bolt, Ernest C., Jr. (R), 882
- "Bolívar en el tiempo," by Cuevas Cancino, 1416
- Bolívar, Simón, 1416
- Bond, Gordon C. (R), 129
- "The Bonds of Wickedness," by Essig, 196
- Bonelli, Franco, editor, "Acciaio per l'industrializzazione: Contributi allo studio del problema siderurgico italiano" (E), 574

- Bonett, Margarita Moreno, "Nacionalismo Novohispano: Mariano Veytia; Historia Antigua, Fundación de Puebla, Guadalupeanismo," 1188
 Bonner, Philip, "Kings, Commoners, and Concessionaires: The Evolution and Dissolution of the Nineteenth-Century Swazi State," 825
 "Les bonnes villes de France du XIV^e au XVI^e Siècle," by Chevalier, 753
 Boog, Horst, *et al.*, "Das Deutsche Reich und der Zweite Weltkrieg," vol. 4, "Der Angriff auf die Sowjetunion," 1353
 "Booker T. Washington," by Harlan, 218
 Boon, James A., "Other Tribes, Other Scribes: Symbolic Anthropology in the Comparative Study of Cultures, Histories, Religions, and Texts," 410
 Booraem, Hendrik, V., "The Formation of the Republican Party in New York: Politics and Conscience in the Antebellum North," 1391
 Borah, Woodrow, "Justice by Insurance: The General Indian Court of Colonial Mexico and the Legal Aides of the Half-Real," 1418
 Borchardt, Knut, "Wachstum, Krisen, Handlungsspielräume der Wirtschaftspolitik: Studien zur Wirtschaftsgeschichte des 19. und 20. Jahrhunderts," 465
 "Bordeaux et le sud-ouest au XVIII^e siècle," by Poussou, 1083
 "A Borderlands Town in Transition," by Hinojosa, 1150
 Boren, Henry C. (R), 741
 Borg, Dorothy, 884
 Boritt, Gabor S. (R), 1395
 Bornstein, Stephen, *et al.*, editors, "The State in Capitalist Europe: A Casebook" (E), 1428
 Borthwick, Sally, "Education and Social Change in China: The Beginnings of the Modern Era," 500
 Borza, Eugene N., and W. Lindsay Adams, editors, "Philip II, Alexander the Great, and the Macedonian Heritage" (E), 243
 Bossy, John, editor, "Disputes and Settlements: Law and Human Relations in the West" (E), 1425
 Boswell, John Eastburn (R), 421
 Boswell, John Eastburn, *Expositio and Oblatio: The Abandonment of Children and the Ancient and Medieval Family*, 10–33
 Bosworth, Richard, "Italy and the Approach of the First World War," 801
 "Les bourgeois de Rouen," by Chaline, 134
 Bousquet, Pierre, *et al.*, "Histoire de l'administration de l'enseignement en France, 1789–1981" (E), 246
 Bowden, Henry Warner (R), 208
 Bowen, Roger W., "Rebellion and Democracy in Meiji Japan: A Study of Commoners in the Popular Rights Movement," 187
 Bowersock, G. W., "Roman Arabia," 1060
 Bowler, Peter J., "The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900," 402
 Bowman, John, "De Valera and the Ulster Question, 1917–1973," 449
 Boxer, C. R., "João de Barros: Portuguese Humanist and Historian of Asia," 459
 Boyajian, James C. (R), 459
 Boyd, Belle, 853
 Boyd, Lois A., and R. Douglas Brackenridge, "Presbyterian Women in America: Two Centuries of a Quest for Status," 862
 Boyer, M. Christine, "Dreaming the Rational City: The Myth of American City Planning," 1176
 Boyer, William W., "America's Virgin Islands: A History of Human Rights and Wrongs," 1423
 Boyle, Marjorie O'Rourke, "Rhetoric and Reform: Erasmus' Civil Dispute with Luther," 1098
 Bozeman, T. Dwight (R), 1385
 Brackenridge, R. Douglas, and Lois A. Boyd, "Presbyterian Women in America: Two Centuries of a Quest for Status," 862
 Bradley, K. R. (R), 1314
 Bradshaw, Brendan (R), 111
 Braestrup, Peter, editor, "Vietnam as History: Ten Years after the Paris Peace Accords" (E), 1202
 Brandes, Joseph (R), 225
 Brantley, Cynthia, "The Giriama and Colonial Resistance in Kenya, 1800–1920," 824
 Brantlinger, Patrick, "Bread and Circuses: Theories of Mass Culture as Social Decay," 1305
 Braude, Benjamin (R), 1127
 Braude, Benjamin, and Bernard Lewis, editors, "Christians and Jews in the Ottoman Empire: The Functioning of a Plural Society," vol. 1, "The Central Lands"; vol. 2, "The Arabic-Speaking Lands" (E), 1199
 Braxton, Carter, 846
 Bray, Alan, "Homosexuality in Renaissance England," 421
 "Brazilian Communism, 1935–1945," by Dulles, 1421
 "Bread and Circuses," by Brantlinger, 1305
 Breisach, Ernst (R), 91
 Breisach, Ernst, "Historiography: Ancient, Medieval, and Modern," 1038
 Bremner, Robert H. (R), 1169
 Brenner, Louis (R), 1372
 Brenner, Reuven, "History—The Human Gamble," 1299
 Brenzel, Barbara M., "Daughters of the State: A Social Portrait of the First Reform School for Girls in North America, 1856–1905," 1169
 Briggs, John W. (R), 543
 "Britain and Ireland, 1914–23," by Lawlor, 1082
 "Britain and Revolutionary France," edited by Jones (E), 245
 "Britain, Europe, and the World, 1850–1982," by Porter, 772
 "Britain's Withdrawal from the Middle East, 1947–1971," by Abadi, 776
 "The British between the Superpowers, 1945–1950," by Barker, 1331
 "British Cinema History," edited by Curran and Porter (E), 897
 "British Economic Growth, 1856–1973," by Matthews *et al.*, 1077
 "The British Labour Movement and Zionism, 1917–1948," by Gorny, 441
 "British Paramountcy," by Kamarkar, 1138
 "British Policy in Palestine, 1917–1922," by McTague, 1366
 "British Policy towards the Ottoman Empire, 1908–1914," by Heller, 773
 "British Propaganda during the First World War, 1914–18," by Sanders and Taylor, 1081
 "British Public Policy, 1776–1939," by Checkland, 437
 "The British Wool Textile Industry, 1770–1914," by Jenkins and Ponting, 1074
 Brody, David (R), 865
 Bronder, Saul E., "Social Justice and Church Authority: The Public Life of Archbishop Robert E. Lucey," 222
 Brooke, C. N. L., *et al.*, editors, "Studies in Numismatic Method: Presented to Philip Grierson" (E), 569
 Brooke, Sir James, 837
 Brooks, John Langdon, "Just before the Origin: Alfred Russel Wallace's Theory of Evolution," 1297

- Brooks, Peter Newman, editor, "Seven-Headed Luther: Essays in Commemoration of a Quincentenary, 1483–1983," 1346
- Brower, Daniel R. (R), 489
- Brown, Charlene J., and Bill F. Chamberlin, editors, "The First Amendment Reconsidered: New Perspectives on the Meaning of Freedom of Speech and Press" (E), 253
- Brown, D. Clayton (R), 224
- Brown, Imogene E., "American Aristides: A Biography of George Wythe," 514
- Brown, John, 520
- Brown, Judith C. (R), 799
- Brown, R. Allen, editor, "Anglo-Norman Studies, V" (E), 895
- Brown, R. Allen, editor, "Proceedings of the Battle Conference on Anglo-Norman Studies," vol. 4, "1981" (E), 245
- Brown, Richard D. (R), 1052
- Brown, Richard Harvey (R), 1298
- Brown, Stewart J., "Thomas Chalmers and the Godly Commonwealth in Scotland," 447
- Browne, Gary L. (R), 1399
- Browne, Janet, "The Secular Ark: Studies in the History of Biogeography," 100
- Browning, Robert S., III, "Two if by Sea: The Development of American Coastal Defense Policy," 1154
- Bruce, Dickson D., Jr. (R), 850
- Bruce, Dickson D., Jr., "The Rhetoric of Conservatism: The Virginia Convention of 1829–30 and the Conservative Tradition in the South," 518
- Brun, Patrice, "Eisphora-Syntaxis-Stratitika: Recherches sur les finances militaires d'Athènes au IV^e siècle av. J.-C.," 1312
- Brundage, Avery, 1309
- Brune, Lester H. (R), 876
- Brunschwig, Henri, 251
- Bryan, William Jennings, 220
- Bryce, Judith, "Cosimo Bartoli, 1503–1572: The Career of a Florentine Polymath," 1108
- Buccellati, Giorgio (R), 1054
- Buck, Mark, "Politics, Finance, and the Church in the Reign of Edward II: Walter Stapeldon, Treasurer of England," 1322
- Buckman, Joseph, "Immigrants and the Class Struggle: The Jewish Immigrant in Leeds, 1880–1914," 1079
- BUDDHISM: Gunawardana, "Robe and Plough," 188
- Buel, Joy Day, and Richard Buel, Jr., "The Way of Duty: A Woman and Her Family in Revolutionary America," 1147
- Buel, Richard, Jr., and Joy Day Buel, "The Way of Duty: A Woman and Her Family in Revolutionary America," 1147
- Bugeaud, Thomas-Robert, 133
- Buhle, Mari Jo, "Women and American Socialism, 1870–1920," 212
- "Building a New American State," by Skowronek, 213
- "Building an Antislavery Wall," by Blackett, 1159
- "Building Character in the American Boy," by MacLeod, 1169
- Bulka, Reuven P., editor, "Dimensions of Orthodox Judaism" (E), 900
- Bullion, John L., "A Great and Necessary Measure: George Grenville and the Genesis of the Stamp Act, 1763–1765," 119
- Bullough, Vern L. (R), 1322
- "Der Bund der Landwirte in der Ersten Tschechoslowakischen Republik," by Linz, 167
- "Bureaucratic Reform in Provincial China," by Ocko, 182
- Burgoyne, John, 428
- "Buried Unsung," by Papanikolas, 229
- Burke, John G., editor, "The Uses of Science in the Age of Newton" (E), 896
- Burns, E. Bradford (R), 240; 1190
- Burns, Thomas S. (R), 415
- Burridge, Trevor (R), 127
- "The Burthen, the Risk, and the Glory," by Tarling, 837
- Buschkühl, Matthias, "Great Britain and the Holy See, 1746–1870," 1068
- Bush, M. L., "The European Nobility," vol. 1, "Noble Privilege," 1293
- Bushkovitch, Paul, and Ivo Banac, editors, "The Nobility in Russia and Eastern Europe," 807
- Bushnell, Amy Turner (R), 786
- Bushnell, David, "Reform and Reaction in the Platine Provinces, 1810–1852," 1420
- BUSINESS AND ENTREPRENEURIAL HISTORY: Benson, "The Penny Capitalists," 1075; Caracciolo, "L'Albero dei Belloni," 477; Clark, "La Rochelle and the Atlantic Economy during the Eighteenth Century," 779; Das Gupta, "Indian Merchants and the Decline of Surat, c. 1700–1750," 829; Davis, "Challenging Colonialism," 498; Fruin, "Kikkoman," 1137; Honeyman, "Origins of Enterprise," 435; Kicza, "Colonial Entrepreneurs," 1189; Kurgan-van Hentenryk, "Rail, finance, et politique," 787; Lindley, "Haciendas and Economic Development," 1189; Loveday, "The Rise and Decline of the American Cut Nail Industry," 1399; Maxwell and Baker, "Sawdust Empire," 527; Miner, "The Rebirth of the Missouri Pacific, 1956–1983," 1410; Monaghan, "A Common Heritage," 202; Owen, "Capitalism and Politics in Russia," 1117; Romano, "Giuseppe Volpi et l'Italie moderne," 800; Spooner, "Risks at Sea," 1092; Tebbutt, "Making Ends Meet," 433; Waller, "The Dukeries Transformed," 1330; Walsh, "The Rise of the Midwestern Meat Packing Industry," 529; Whitten, "The Emergence of Giant Enterprise, 1860–1914," 1400
- "Business and Its Environment," edited by Sharlin (E), 253
- Butcher, John G. (R), 1140
- Button, H. Warren (R), 1408
- Button, H. Warren, and Eugene F. Provenzo, Jr. (C), 1222
- Byman, Seymour (R), 758
- Byrnes, Robert F. (R), 1123
- Byrnes, Robert F., "Awakening American Education to the World: The Role of Archibald Cary Coolidge, 1866–1928," 544
- Byrnes, Robert F., editor, "After Brezhnev: Sources of Soviet Conduct in the 1980s," 496
- Bythell, Duncan (R), 1074
- "The Byzantine State Finances in the Eighth and Ninth Centuries," by Treadgold, 110
- Caesar, Julius, 741
- Cahen, Claude, "Orient et occident au temps des croisades," 751
- Calabria, Antonio (R), 1354
- "A Calculating People," by Cohen, 203
- Calder, Bruce (R), 562
- Calhoon, Robert M. (R), 1388
- Calhoun, Charles W. (R), 526
- Calhoun, Craig (R), 1074

- "California Farmland," by Liebman, 856
- Callomp, Alain, "La maison du père: Famille et village en Haute-Provence aux XVII^e et XVIII^e siècles," 1334
- Calnek, Edward E. (R), 1417
- Calvert, Peter, "The Concept of Class: An Historical Introduction," 98
- Calvert, Robert A. (R), 1162
- "The Cambridge Ancient History," vol. 3, pt. 1, "The Prehistory of the Balkans; and the Middle East and the Aegean World, Tenth to Eighth Centuries B.C.," edited by Boardman *et al.*, 1054
- "The Cambridge Economic History of India," vol. 2, "c. 1757–c. 1970," edited by Kumar and Desai, 829
- "The Cambridge History of China," vol. 12, pt. 1, "Republican China, 1912–1949," edited by Fairbank, 1133
- "The Cambridge History of Iran," vol. 3, "The Seleucid, Parthian, and Sasanian Periods," edited by Yarshater, 1055
- Cameron, Averil (R), 106
- Campbell, Ballard C. (R), 214
- Campbell, Ballard C., "Representative Democracy: Public Policy and Midwestern Legislatures in the Late Nineteenth Century," 530
- Campbell, John C. (R), 481
- Campbell, Randolph B., "A Southern Community in Crisis: Harrison County, Texas, 1850–1880," 1397
- Campbell, Rex R., and Daniel M. Johnson, "Black Migration in America: A Social Demographic History," 542
- "Canada and the Constitution, 1979–1982," by McWhinney, 559
- Câncea, Paraschiva, *et al.*, editors, "Istoria parlamentului și a vieții parlamentare din România până la 1918," 1110
- Canny, Nicholas (R), 129
- Cantilupe, St. Thomas, 245
- Cantor, Milton (R), 191
- CAPITALISM: Aymard, editor, "Dutch Capitalism and World Capitalism," 789; Benson, "The Penny Capitalists," 1075; Foweraker, "The Struggle for Land," 1193; Kurgan-van Hentenryk, "Rail, finance, et politique," 787; Mallon, "The Defense of Community in Peru's Central Highlands," 890; Miliband, "Capitalist Democracy in Britain," 438
- "Capitalism and Politics in Russia," by Owen, 1117
- "Capitalism, Slavery, and Republican Values," by Kaufman, 520
- "Capitalist Democracy in Britain," by Miliband, 438
- Caracciolo, Alberto, "L'Albero dei Belloni: Una dinastia di mercanti del Settecento," 477
- Cardoso, Gerald, "Negro Slavery in the Sugar Plantations of Veracruz and Pernambuco, 1550–1680," 560
- Cardoso, Lawrence A. (R), 544
- Cardoza, Anthony L., "Agrarian Elites and Italian Fascism: The Province of Bologna, 1901–1926," 160
- Carey, Patrick W. (R), 1151
- Carey, Patrick, "An Immigrant Bishop: John England's Adaptation of Irish Catholicism to American Republicanism," 851
- Carey, Peter (R), 510
- "Caribbean Migrants," by Richardson, 562
- "Carl Schmitt," by Bendersky, 152
- Carley, Michael Jabara, "Revolution and Intervention: The French Government and the Russian Civil War, 1917–1919," 1337
- Carlile, Richard, 432
- Carls, Alice-Catherine (R), 806
- Carlton, Charles, "Charles I: The Personal Monarch," 1066
- Carlton, David L., "Mill and Town in South Carolina, 1880–1920," 210
- "Carolingian Essays," edited by Blumenthal, 752
- Carr, E. H., "Twilight of the Comintern, 1930–1935," 815
- Carrasco, David, "Quetzalcoatl and the Irony of Empire: Myths and Prophecies in the Aztec Tradition," 1417
- Carreira, António, "The People of the Cape Verde Islands: Exploitation and Emigration," 179
- Carstensen, Fred V., and Gregory Guroff, editors, "Entrepreneurship in Imperial Russia and the Soviet Union" (E), 1200
- "Carter Braxton, Virginia Signer," by Dill, 846
- Carus-Wilson, E. M., 1427
- "The Casablanca Connection," by Hoisington, 1339
- Casdorff, Paul D. (R), 1144
- Case, John, 757
- "A Case of Black and White," by Rothschild, 230
- Cassels, Alan (R), 477
- "A Caste in a Changing World," by Conlon, 503
- "Catholic Royalism in the Department of the Gard, 1814–1852," by Fitzpatrick, 783
- CATHOLICISM: Bronder, "Social Justice and Church Authority," 222; Carey, "An Immigrant Bishop," 851; Cushner, "Jesuit Ranches and the Agrarian Development of Colonial Argentina, 1650–1767," 1419; Fitzpatrick, "Catholic Royalism in the Department of the Gard, 1814–1852," 783; Frazee, "Catholics and Sultans," 1127; Freedman, "The Diocese of Vic," 1319; Hayman, "Catholicism in Rhode Island and the Diocese of Providence, 1780–1886," 1151; Hibbard, "Charles I and the Popish Plot," 760; Kantowicz, "Corporation Sole," 221; Kee, "Miracle in the Early Christian World," 1292; Kerr, "Peel, Priests, and Politics," 449; Miller and Wakelyn, editors, "Catholics in the Old South," 850; Raem, "Katholischer Gesellenverein und deutsche Kolpingsfamilie in der Ära des Nationalsozialismus," 145; Schoenl, "The Intellectual Crisis in English Catholicism," 441; Smith, "The Church and Politics in Chile," 1193; Sutton, "Nationalism, Positivism, and Catholicism," 140
- "Catholicism in Rhode Island and the Diocese of Providence, 1780–1886," by Hayman, 1151
- "Catholics and Sultans," by Frazee, 1127
- "Catholics in the Old South," edited by Miller and Wakelyn, 850
- Ceausescu, Ilie, editor, "File din istoria militară a poporului Român: Studii" (E), 576
- Ceausescu, Ilie, editor, "War, Revolution, and Society in Romania: The Road to Independence" (E), 576
- Censer, Jack R. (R), 452
- "Censorship in Imperial Japan," by Mitchell, 827
- "Central America," edited by Feinberg (E), 254
- "The Central Lands," vol. 1 of "Christians and Jews in the Ottoman Empire," edited by Braude and Lewis (E), 1199
- Centre de recherche d'histoire sociale de l'université de picardie, "Oisiveté et loisirs dans les sociétés occidentales au XIX^e siècle" (E), 1198
- Cerullo, John J., "The Secularization of the Soul: Psychical Research in Modern Britain," 444
- Cervo, Amado Luiz, "O parlamento brasileiro e as relações exteriores, 1826–1889," 240
- "I ceti dirigenti dell'età comunale nei secoli XII e XIII," by Comitato di studi sulla storia dei ceti dirigenti in Toscana *et al.* (E), 248
- "I ceti dirigenti in Toscana nell'età precomunale," by

- Associazione Toscana della nobiltà Italiana *et al.* (E), 248
- "Chacabuco y Vergara," by Marín, 1192
- Chadwick, Owen, "Hensley Henson: A Study in the Friction between Church and State," 1080
- Chalfant, Edward (C), 589
- Chaline, Jean-Pierre, "Les bourgeois de Rouen: Une élite urbaine au XIX^e siècle," 134
- Challener, Richard D. (R), 517
- "Challenging Colonialism," by Davis, 498
- Chalmers, Thomas, 447
- Chamberlain, Muriel E., "Lord Aberdeen: A Political Biography," 768
- Chamberlin, Bill F., and Charlene J. Brown, editors, "The First Amendment Reconsidered: New Perspectives on the Meaning of Freedom of Speech and Press" (E), 253
- Chambers, Mortimer (R), 1312
- Chan, Anthony B., "Arming the Chinese: The Western Armaments Trade in Warlord China, 1920–1928," 183
- Chan, Anthony B., "Gold Mountain: The Chinese in the New World," 886
- "The Chancelleries of Europe," by Palmer, 1045
- Chandler, David P., "A History of Cambodia," 834
- Chaney, William A. (R), 417
- Chang, K. C., "Art, Myth, and Ritual: The Path to Political Authority in Ancient China," 1373
- "Changes in the Land," by Cronon, 839
- "The Changing Fenland," by Darby, 755
- "The Changing of the Gods," by Manuel, 1047
- "Charity and 'Bienfaisance,'" by Jones, 132
- "Charles I," by Carlton, 1066
- "Charles I and the Popish Plot," by Hibbard, 760
- "Charles Willson Peale and His World," by Richardson *et al.*, 847
- Charlot, Jean, "Le gaullisme d'opposition, 1946–1958: Histoire politique du gaullisme," 784
- Chase, Philander D. (R), 846
- Chase, William C., "The American Law School and the Rise of Administrative Government," 875
- Checkland, Sydney, "British Public Policy, 1776–1939: An Economic, Social, and Political Perspective," 437
- Chejne, Anwar G., "Islam and the West: The Moriscos: A Cultural and Social History," 1064
- "Chen Duxiu," by Feigon, 1375
- Chénétier, Marc, and Rob Kroes, editors, "Impressions of a Gilded Age: The American Fin de Siècle" (E), 900
- Cherepnin, L. V., "Voprosy metodologii istoricheskogo issledovaniia: Teoreticheskie problemy istorii feodalizma," 486
- Chernaik, Warren L., "The Poet's Time: Politics and Religion in the Work of Andrew Marvell," 425
- "Cherokees and Missionaries, 1789–1839," by McLoughlin, 1390
- Cherry, Martin (R), 1071
- Chessman, G. Wallace (R), 1409
- Chester, Edward W., "United States Oil Policy and Diplomacy: A Twentieth-Century Overview," 884
- Chevalier, Bernard, "Les bonnes villes de France du XIV^e au XVI^e Siècle," 753
- "Child of Conflict," edited by Cumings (E), 254
- "China among Equals," edited by Rossabi (E), 577
- "China and Charles Darwin," by Pusey, 1135
- "China Turned Rightside Up," by Thaxton, 184
- "Chinese Society in Rural Malaysia," by Siaw, 1140
- "Chinese Women," by Yao, 826
- Chinnici, Joseph P. (R), 1068
- Chirot, Daniel (R), 163
- Choate, Rufus, 1155
- "The Chosen People in America," by Eisen, 1178
- "The Chouans," by Sutherland, 780
- Chouet, Jean-Robert, 474
- "Les chrétiens face au nazisme et au stalinisme," by de Montclos, 1306
- Christ, Karl, "Römische Geschichte und deutsche Geschichtswissenschaft," 105
- "Christians and Jews in the Ottoman Empire: The Functioning of a Plural Society," vol. 1, "The Central Lands"; vol. 2, "The Arabic-Speaking Lands," edited by Braude and Lewis (E), 1199
- Christoph, James B. (R), 438
- "Christopher Gadsden and the American Revolution," by Godbold and Woody, 515
- Chrysostom, John, 1059
- Chu, Wen Djang (R), 182
- Chubb, Basil, "The Government and Politics of Ireland," 777
- "The Church and Politics in Chile," by Smith, 1193
- "The Church and War," edited by Sheils (E), 1195
- Church, Clive H., "Europe in 1830: Revolution and Political Change," 406
- Church, Clive H., "Revolution and Red Tape: The French Ministerial Bureaucracy, 1770–1850," 452
- Churchill, Frederick B. (R), 1297
- Churchill, Stacy (R), 790
- Churchill, Winston, 125
- "Churchill's *World Crisis* as History," by Prior, 125
- Cienciala, Anna M. (R), 774
- "City Building in the New South," by Platt, 209
- "City of Blood Revisited," by Home, 180
- "City, Town, and Countryside in the Early Byzantine Era," edited by Hohlfelder, 744
- "Clarendon and the English Revolution," by Harris, 1326
- Clark, Abraham, 194
- Clark, Charles E. (R), 847
- Clark, John G., "La Rochelle and the Atlantic Economy during the Eighteenth Century," 779
- Clark, Michael D. (R), 202
- Clark, Michael D., "Coherent Variety: The Idea of Diversity in British and American Conservative Thought," 97
- Clark, Samuel, and James S. Donnelly, Jr., editors, "Irish Peasants: Violence and Political Unrest, 1780–1914" (E), 897
- Clarkson, L. A. (R), 435
- Clary, David A. (R), 534
- Clasen, Claus-Peter (R), 1295
- "Class, State, and Industrial Structure," by Weaver, 1186
- "Classical, Medieval, and Renaissance Foundations of Corasius' Systematic Methodology," vol. 2 of "Origins of Legislative Sovereignty and the Legislative State," by Fell, 1296
- Clay, Lucius DuBignon, 796
- Clegg, Hugh (R), 445
- Clements, Kendrick A., *Herbert Hoover and Conservation, 1921–33*, 67–88
- Clements, Kendrick A., "William Jennings Bryan: Missionary Isolationist," 220
- "Clifford W. Beers," by Dain, 548
- Cline, Catherine Ann (R), 441
- Clinton, Catherine, "The Plantation Mistress: Woman's World in the Old South," 196
- Clinton, Kevin (R), 1057
- "Clio and the Bitch Goddess," by Bogue (E), 1202
- "Clipped Wings," by Horwich, 553
- "Cloth and Clothing in Medieval Europe," edited by Harte and Ponting (E), 1427
- Coakley, Robert W. (R), 869
- Cobb, Richard, "French and Germans, Germans and

- French: A Personal Interpretation of France under Two Occupations, 1914–1918/1940–1944," 140
- Cobb, Thomas R. R., 1392
- Cochran, Thomas C., 253
- Cockshaw, P., "Le personnel de la chancellerie de Bourgogne-Flandre sous les ducs de Bourgogne de la maison de Valois, 1384–1477," 114
- "Coffee and the Growth of Agrarian Capitalism in Nineteenth-Century Puerto Rico," by Bergad, 888
- Cohen, Amnon, "Political Parties in the West Bank under the Jordanian Regime, 1949–1967," 176
- Cohen, David, "Theft in Athenian Law," 1057
- Cohen, Gary B. (R), 166; 1104
- Cohen, I. Bernard (R), 763
- Cohen, Lester H. (R), 194
- Cohen, Michael J., "Palestine and the Great Powers, 1945–1948," 177
- Cohen, Patricia Cline, "A Calculating People: The Spread of Numeracy in Early America," 203
- Cohen, Shaye J. D. (R), 1315
- Cohen, Stanley, and Andrew Scull, editors, "Social Control and the State," 1305
- Cohen, Stuart A. (R), 1366
- Cohen, Warren I., editor, "New Frontiers in American–East Asian Relations: Essays Presented to Dorothy Borg," 884
- "Coherent Variety," by Clark, 97
- "The Cohesion of Saudi Arabia," by Helms, 819
- "The Coinage of the Lycian League," by Troxell, 1313
- Colaïaco, James A., "James Fitzjames Stephen and the Crisis of Victorian Thought," 122
- COLD WAR: Guilbaut, "How New York Stole the Idea of Modern Art," 1177; Leffler, *The American Conception of National Security and the Beginnings of the Cold War, 1945–48* (Forum), 346–81; Ninkovich, "The Diplomacy of Ideas," 883
- Colden, Cadwallader II, 1388
- Cole, Bernard D., "Gunboats and Marines: The United States Navy in China, 1925–1928," 538
- Cole, David C., and Yung Chul Park, "Financial Development in Korea, 1945–1978," 502
- Cole, Wayne S., "Roosevelt and the Isolationists, 1932–45," 538
- Colish, Marcia L. (R), 107
- Collier, George A., et al., editors, "The Inca and Aztec States, 1400–1800: Anthropology and History," 559
- Collier, John, 870
- Collins, Robert O., "Shadows in the Grass: Britain in the Southern Sudan, 1918–1956," 824
- Collinson, Patrick (C), 590
- Collinson, Patrick, "The Religion of Protestants: The Church in English Society, 1559–1625," 761
- "Colonial Entrepreneurs," by Kicza, 1189
- "Colonial Rule and Regional Imbalance in Central Africa," by Fetter, 821
- "Colonial South Carolina," by Weir, 511
- COLONIALISM: Anna, "Spain and the Loss of America," 1090; Bayly, "Rulers, Townsmen, and Bazaars," 504; Bullion, "A Great and Necessary Measure," 119; Fetter, "Colonial Rule and Regional Imbalance in Central Africa," 821; Grandin, "Le Soudan nilotique et l'administration britannique, 1898–1956," 124; Heussler, "Completing a Stewardship," 1379; Hoisington, "The Casablanca Connection," 1339; Keppie, "Colonisation and Veteran Settlement in Italy, 47–14 B.C.," 413; Liss, "Atlantic Empires," 92; McCarthy, "Social Change and the Growth of British Power in the Gold Coast," 1130; Persell, "The French Colonial Lobby, 1889–1938," 1086
- "Colonisation and Veteran Settlement in Italy, 47–14 B.C.," by Keppie, 413
- Combs, Jerald A., "American Diplomatic History: Two Centuries of Changing Interpretations," 234
- "The Coming of Industrial Order," by Prude, 1157
- Comitato di studi sulla storia dei ceti dirigenti in Toscana et al., "I ceti dirigenti dell'età comunale nei secoli XII e XIII" (E), 248
- "Commerce and Social Standing in Ancient Rome," by D'Arms, 740
- "Committee of Vigilance," by Levi, 230
- "A Common Heritage," by Monaghan, 202
- "The Common Lawyers of Pre-Reformation England," by Ives, 756
- COMMUNISM: Anderson, "Fascists, Communists, and the National Government," 447; Carr, "Twilight of the Comintern, 1930–1935," 815; Dulles, "Brazilian Communism, 1935–1945," 1421; Feigon, "Chen Duxiu," 1375; Jelinek, "The Lust for Power," 1113; Kinner, "Marxistische deutsche Geschichtswissenschaft, 1917 bis 1933," 468; MacFarquhar, "The Origins of the Cultural Revolution," vol. 2, "The Great Leap Forward, 1958–1960," 1136; Naison, "Communists in Harlem during the Depression," 1174; Sandford, "From Hitler to Ulbricht," 473; Shue, "Peasant China in Transition," 185; Wall, "French Communism in the Era of Stalin," 1087
- "Communists in Harlem during the Depression," by Naison, 1174
- "Community, Class, and Careerism," by Bennett, 112
- COMPARATIVE HISTORY: Christ, "Römische Geschichte und deutsche Geschichtswissenschaft," 105; Goody, "The Development of the Family and Marriage in Europe," 1050; Himka, "Socialism in Galicia," 1114; Kautsky, "The Politics of Aristocratic Empires," 1043; Keil and Jentz, editors, "German Workers in Industrial Chicago, 1850–1910," 1164; Kitsikis, "Istoria tou Ellinotourkikou Horou apo ton E. Venizelo ston g. Papadopoulos, 1928–1973," 803; Kitsikis, "Synkritiki Istoria Ellados kai Tourkias stou 20% aiona," 803; Moisse, "Land Reform in China and North Vietnam," 184; Raeff, "The Well-Ordered Police State," 404; Wolf, "Europe and the People without History," 89
- "Completing a Stewardship," by Heussler, 1379
- Conacher, J. B. (R), 1071
- "The Concept of Class," by Calvert, 98
- Cone, Carl B. (R), 766
- Cone, John Frederick, "First Rival of the Metropolitan Opera," 1162
- Confer, Vincent (R), 1086
- "Conflict and Accommodation," by Nash, 1164
- "Confucianism and Autocracy," by Dardess, 1134
- Congdon, Lee, "The Young Lukács," 482
- "Congress Declares War," by Hatzenbuehler and Ivie, 1391
- Conlon, Frank F., "A Caste in a Changing World: The Chitrapur Saraswat Brahmins, 1700–1935," 503
- Connelly, Thomas L. (R), 844
- Conner, Valerie Jean, "The National War Labor Board: Stability, Social Justice, and the Voluntary State in World War I," 865
- Conniff, Michael L., editor, "Latin American Populism in Comparative Perspective," 564
- Conrad, Christoph, and Hans-Joachim von Kondratowitz, editors, "Gerontologie und Sozialgeschichte: Wege zu einer historischen Betrachtung des Alters" (E), 898
- "Conscience and Convenience," by Rothman, 227
- "Consensus fidelium," by Hannig, 419

- CONSERVATION AND THE ENVIRONMENT: Clements, Herbert Hoover and *Conservation*, 1921–33, 67–88; Cowdrey, "This Land, This South," 839; Darby, "The Changing Fenland," 755; Doughty, "Wildlife and Man in Texas," 1180; Dunbar, "Forging New Rights in Western Waters," 838; Engel, "Sacred Sands," 535; Flader, editor, "The Great Lakes Forest," 535; Fox, "John Muir and His Legacy," 223; Rackham, "Ancient Woodland," 755; Richardson, "David T. Mason, Forestry Advocate," 1182; Schrepfer, "The Fight to Save the Redwoods," 534; Thomas, "Man and the Natural World," 733; Tober, "Who Owns the Wildlife?" 855; Twight, "Organizational Values and Political Power," 1181
- CONSERVATISM: Bruce, "The Rhetoric of Conservatism," 518; Clark, "Coherent Variety," 97; Dowler, "Dostoevsky, Grigor'ev, and Native Soil Conservatism," 169; Kolkey, "The New Right, 1960–1968, with Epilogue, 1969–1980," 1414; Reinhard, "The Republican Right since 1945," 1413
- "Conservative Reformer, 1804–1848," vol. 1 of "Joseph Howe," by Beck, 235
- "The Conspiracy and Death of Lin Biao," by Ming-Le, 500
- CONSTITUTIONAL HISTORY: Căncea *et al.*, editors, "Istoria parlamentului și a vieții parlamentare din România până la 1918," 1110; Heinig, "Reichsstädte, freie Städte und Königtum, 1389–1450," 1345; McWhinney, "Canada and the Constitution, 1979–1982," 559; Onuf, "The Origins of the Federal Republic," 846
- "Contemporary American Immigration," vol. 1, "European"; vol. 2, "Non-European," edited by Cuddy (E), 1431
- "The Contested City," by Mollenkopf, 1175
- Contosta, David R. (R), 219
- Contreni, John J. (R), 1063
- The Contribution of Women to Modern Historiography in Great Britain, France, and the United States, 1750–1940*, by Smith, 709–32
- Conway, John S. (R), 145
- Coolidge, Archibald Cary, 544
- Cooper, Jerry M., "The Army and Civil Disorder: Federal Military Intervention in Labor Disputes, 1877–1900," 528
- Cooper, John Milton, Jr., "The Warrior and the Priest: Woodrow Wilson and Theodore Roosevelt," 1170
- Cooper, Matthew, and Margaret Rodman, editors, "The Pacification of Melanesia" (E), 1201
- Cooperman, Bernard Dov, editor, "Jewish Thought in the Sixteenth Century" (E), 1195
- Cope, Esther S. (R), 760
- "Corasius and the Renaissance Systematization of Roman Law," vol. 1 of "Origins of Legislative Sovereignty and the Legislative State," by Fell, 1296
- Corn, Joseph J., "The Winged Gospel: America's Romance with Aviation, 1900–1950," 876
- "Corporation Sole," by Kantowicz, 221
- "La corte nella cultura e nella storiografia," edited by Mazzaelli and Olmi (E), 1429
- Cosgrove, Art, and J. I. McGuire, editors, "Parliament and Community" (E), 1198
- Cosgrove, Richard A. (R), 437
- "Cosimo Bartoli, 1503–1572," by Bryce, 1108
- "Cossack Rebellions," by Gordon, 168
- Cott, Nancy F. (R), 1382
- Coughtry, Jay, "The Notorious Triangle: Rhode Island and the African Slave Trade, 1700–1807," 841
- "Covenants without the Sword," by Kyba, 775
- Cowan, Ian B., and Duncan Shaw, editors, "The Renaissance and Reformation in Scotland: Essays in Honour of Gordon Donaldson" (E), 572
- Coward, Barry, "The Stanleys: Lords Stanley and Earls of Derby, 1385–1672; The Origins, Wealth, and Power of a Landowning Family," 1071
- Cowdrey, Albert E., "This Land, This South: An Environmental History," 839
- Cox, Thomas C. (R), 1396
- Cox, Thomas C., "Blacks in Topeka, Kansas, 1865–1915: A Social History," 859
- "Craftsmen and Interior Decoration in England, 1660–1820," by Beard, 427
- Crane, Robert I. (R), 506
- Craton, Michael, "Testing the Chains: Resistance to Slavery in the British West Indies," 561
- "Creación del consejo de hacienda de Castilla, 1523–1525," by Hernández Esteve, 1341
- "The Creation of Modern Georgia," by Bartley, 1144
- "Credit and Socioeconomic Change in Colonial Mexico," by Greenow, 238
- Cress, Lawrence Delbert (R), 1145
- CRIME, CRIMINALS, AND THE POLICE: Harring, "Policing a Class Society," 1401; Joselit, "Our Gang," 1179; Keve, "The McNeil Century," 1166; Lüdtke, "'Gemeinwohl,' Polizei und Festungspraxis," 146; Sturma, "Vice in a Vicious Society," 1142
- "Las crisis de mortalidad en la España interior, siglos XVI–XIX," by Pérez Moreda, 458
- "Crisis in Consciousness," by Rubanowice, 791
- "The Crisis of Austrian Socialism," by Rabinbach, 160
- "The Crisis of Medieval Russia, 1200–1304," by Fennell, 487
- "A Critical History of the USSR," vol. 1, "The Legacy of the Bolshevik Revolution," by Rousset, 495
- Croke, Brian, and Alanna M. Emmett, editors, "History and Historians in Late Antiquity" (E), 1426
- Cronin, James E., and Carmen Sirianni, editors, "Work, Community, and Power: The Experience of Labor in Europe and America, 1900–1925" (E), 568
- Cronon, William, "Changes in the Land: Indians, Colonists, and the Ecology of New England," 839
- Cross, Gary S., "Immigrant Workers in Industrial France: The Making of a New Laboring Class," 784
- "Crosses on the Ballot," by Wald, 436
- Crossick, Geoffrey (R), 433
- "Crossroads of Decision," by Jablon, 1404
- Crouch, Tom D. (R), 553; 1052
- Crow, Charles L., and Howard Kerr, editors, "The Occult in America: New Historical Perspectives" (E), 899
- Crummey, Robert O., "Aristocrats and Servitors: The Boyar Elite in Russia, 1613–1689," 808
- "Crusaders and Compromisers," edited by Kraut, 1158
- "Cuba between Empires, 1878–1902," by Pérez, 1423
- Cuddy, Dennis Laurence, editor, "Contemporary American Immigration: Interpretive Essays," vol. 1, "European"; vol. 2, "Non-European" (E), 1431
- Cuevas Cancino, Francisco, "Bolívar en el tiempo," 1416
- "The Cult of Power," edited by Held (E), 895
- CULTURAL HISTORY: Banner, "American Beauty," 191; Beard, "Craftsmen and Interior Decoration in England, 1660–1820," 427; Boswell, *Expositio and Oblatio: The Abandonment of Children and the Ancient and Medieval Family*, 10–33; Carrasco, "Quetzalcoatl and the Irony of Empire," 1417; Corn, "The Winged Gospel," 876; Dellheim, "The Face of the

- Past," 123; Fox and Lears, editors, "The Culture of Consumption," 1172; Grant, "From Alexander to Cleopatra," 103; Haber, "Beyond Sixty-Five," 193; Hohlfelder, "City, Town, and Countryside in the Early Byzantine Era, 744; Il'ina, "Kul'turnoe stroitel'stvo v Petrograde, Oktjabr' 1917–1920 gg.," 493; Kern, "The Culture of Time and Space, 1880–1918," 736; Landes, "Revolution in Time," 1052; Levine, *William Shakespeare and the American People: A Study in Cultural Transformation*, 34–66; Lewis, "Main Currents in Caribbean Thought," 887; Major-Poetzl, "Michel Foucault's Archaeology of Western Culture," 90; Martin, "The Hypocrisy of Justice in the Belle Époque," 1337; McCarthy, "Dark Continent," 1369; Mizruchi, "Regulating Society," 410; Morales Carrión *et al.*, "Puerto Rico," 889; Olson, "Science Deified and Science Defied," 99; Ragon, "The Space of Death," 1307; Ryan, "The Empire of the Mother," 205; Sawyer, "Kings and Vikings," 745; Scullard, "Festivals and Ceremonies of the Roman Republic," 739; Septimius, "Hispano-Jewish Culture in Transition," 420; Sidak, "Kroz pet stoljeća Hrvatske povijesti," 1357; Sprandel, "Altersschicksal und Altersmoral," 1060; Taylor, "The Birth of Vietnam," 834; Trexler, "Public Life in Renaissance Florence," 475; Vovelle, "La mort et l'Occident de 1300 à nos jours," 1307; Watson, "Song and Democratic Culture in Britain," 1081; Wheaton, "Savoring the Past," 450; Whisnant, "All That Is Native and Fine," 1183
- "Culture, Conflict, and Coexistence," by Parks, 1123
- "The Culture of Consumption," edited by Fox and Lears, 1172
- "The Culture of Time and Space, 1880–1918," by Kern, 736
- Cumberland, William H., "Wallace M. Short: Iowa Rebel," 1171
- Cumings, Bruce, editor, "Child of Conflict: The Korean-American Relationship, 1943–1953" (E), 254
- Curran, James, and Vincent Porter, editors, "British Cinema History" (E), 897
- Curran, Joseph M. (R), 1082
- Curran, Robert Emmett (R), 851
- Curtin, Patricia Romero (R), 1370
- Curtin, Philip D. (R), 89
- Cushner, Nicholas P., "Jesuit Ranches and the Agrarian Development of Colonial Argentina, 1650–1767," 1419
- Cutter, Donald C. (R), 734
- Dabney, William M. (R), 1394
- "Daily Life," vol. 4 of "A Mediterranean Society," by Goitein, 1365
- Dain, Norman (R), 1167
- Dain, Norman, "Clifford W. Beers: Advocate for the Insane," 548
- Dalgaard, Bruce R., and Richard K. Vedder, editors, "Variations in Business and Economic History: Essays in Honor of Donald L. Kemmerer" (E), 242
- "Dalla città al borgo," by De Maddalena, 799
- Dallas, Gregor, "The Imperfect Peasant Economy: The Loire Country, 1800–1914," 135
- Dallek, Robert, "The American Style of Foreign Policy: Cultural Politics and Foreign Affairs," 541
- Dallin, Alexander (R), 496
- Daly, M. W. (R), 824
- Daly, William M. (R), 1317
- D'Amico, John F., "Renaissance Humanism in Papal Rome: Humanists and Churchmen on the Eve of the Reformation," 1106
- "Dangerous Relations," by Ulam, 174
- Daniel, Cletus E. (R), 866
- Daniels, Bruce C. (R), 1386
- Daniels, Douglas Henry (R), 859
- Daniels, Jonathan, 224
- Daniels, Roger (R), 871
- "Danish Medieval History," edited by Skyum-Nielsen and Lund (E), 249
- "Danmarks historie," vol. 4, "Tiden 1730–1814," by Feldbæk, 461
- "Dansk kolonihistorie," edited by Jensen *et al.* (E), 1429
- Darby, H. C., "The Changing Fenland," 755
- Darby, John, editor, "Northern Ireland: The Background to the Conflict" (E), 897
- Dardess, John W., "Confucianism and Autocracy: Professional Elites in the Founding of the Ming Dynasty," 1134
- "Dark Continent," by McCarthy, 1369
- "Dark Horse," by Neal, 1405
- D'Arms, John H. (R), 413
- D'Arms, John H., "Commerce and Social Standing in Ancient Rome," 740
- Darwin, Charles Robert, 402; 1135
- Darwin, J. G. (R), 776
- Das Gupta, Ashin, "Indian Merchants and the Decline of Surat, c. 1700–1750," 829
- "Daughters of the State," by Brenzel, 1169
- "David T. Mason, Forestry Advocate," by Richardson, 1182
- Davies, Margery W., "Woman's Place Is at the Typewriter: Office Work and Office Workers, 1870–1930," 1165
- Davies, Wendy, "Wales in the Early Middle Ages," 416
- Davis, Eric, "Challenging Colonialism: Bank Misr and Egyptian Industrialization, 1920–1941," 498
- Davis, Richard, "The English Rothschilds," 1077
- Davis, Robert H. (R), 1416
- Davis, Ronald L. F., "Good and Faithful Labor: From Slavery to Sharecropping in the Natchez District, 1860–1890," 521
- Dawn, C. Ernest (R), 1364
- de Barros, Joao, 459
- de Cramaud, Simon, 753
- de Gaulle, Charles, 1340
- De Geer, Hans, "Job Studies and Industrial Relations: Ideas about Efficiency and Relations between the Parties of the Labour Market in Sweden, 1920–1950," 158
- De Grummond, Jane Lucas, "Renato Beluche: Smuggler, Privateer, and Patriot, 1780–1860," 887
- de Jong, L., "Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog," vol. 10b, "Het laatste jaar II," 460
- de la Roncière, Charles-M., "Prix et salaires à Florence au XIV^e siècle, 1280–1380," 418
- De León, Arnoldo (R), 1150
- De León, Arnoldo, "They Called Them Greasers: Anglo Attitudes Toward Mexicans in Texas, 1821–1900," 857
- De Maddalena, Aldo, "Dalla città al borgo: Avvio di una metamorfosi economica e sociale nella Lombardia spagnola," 799
- de Meere, J. M. M., "Economische ontwikkeling en levensstandaard in Nederland gedurende de eerste helft van de negentiende eeuw," 1344
- de Montclos, Xavier, "Les chrétiens face au nazisme et au stalinisme: L'Épreuve totalitaire, 1939–1945," 1306

- de Mun, Count Albert, 455
 De Shazo, Peter, "Urban Workers and Labor Unions in Chile, 1902–1927," 892
 "De Valera and the Ulster Question, 1917–1973," by Bowman, 449
 De Valera, Eamon, 449
 Deak, Istvan (R), 164
 Dean, Warren (R), 1193
 "Death and Renewal," by Hopkins, 741
 "Death in a Promised Land," by Ellsworth, 859
 Debrizzi, John A., "Ideology and the Rise of Labor Theory in America," 863
 "The Decline of the Celtic Languages," by Durkacz, 127
 "Decoding the Past," by Loewenberg, 94
 "The Defence of Malaysia and Singapore," by Wah, 836
 "The Defense of Community in Peru's Central Highlands," by Mallon, 890
 Dellheim, Charles, "The Face of the Past: The Preservation of the Medieval Inheritance in Victorian England," 123
 Dembkowski, Harry E., "The Union of Lublin: Polish Federalism in the Golden Age," 483
 "Die Demagogie des Hitlerfaschismus," by Petzold, 151
 "Demographic Dimensions of the New Republic," by McClelland and Zeckhauser, 201
 DEMOGRAPHY: Acosta Rodríguez, "La población de Luisiana Española, 1763–1803," 843; Carreira, "The People of the Cape Verde Islands," 179; Díaz-Briquets, "The Health Revolution in Cuba," 565; Imhof, "Die gewonnenen Jahre," 409; Johnson and Campbell, "Black Migration in America," 542; Kaelble, "Soziale Mobilität und Chancengleichheit im 19. und 20. Jahrhundert," 1302; Lockridge, "The Fertility Transition in Sweden," 1096; McCaa, "Marriage and Fertility in Chile," 563; McClelland and Zeckhauser, "Demographic Dimensions of the New Republic," 201; Pasternak, "Guests in the Dragon," 1374; Pérez Moreda, "Las crisis de mortalidad en la España interior, siglos XVI–XIX," 458
 Dennis, George T. (R), 1126
 Denovo, John A. (R), 177; 1132
 Dent, C. M., "Protestant Reformers in Elizabethan Oxford," 1065
Depopulation, Nationalism, and Feminism in Fin-de-Siècle France, by Offen, 648–76
 "Depression and New Deal in Virginia," by Heinemann, 868
Depth, Span, and Relevance, by Curtin, 1–9
 Desai, Meghnad, and Dharma Kumar, editors, "The Cambridge Economic History of India," vol. 2, "c. 1757–c. 1970," 829
 Despalatović, Elinor M. (R), 480
 "Deutsche Geschichte, 1800–1866," by Nipperdey, 1099
 "Die deutsche Inflation," edited by Feldman *et al.*, 792
 "Das Deutsche Reich und der Zweite Weltkrieg," vol. 4, "Der Angriff auf die Sowjetunion," edited by Boog *et al.*, 1353
 "Deutsches Kapital in den USA," by Kabisch, 233
 "Die Deutschlandpolitik Frankreichs und die französische Zone, 1945–1949," edited by Scharf and Schröder (E), 899
 "The Developing West," edited by Foster (E), 254
 "The Development of Law on the Rocky Mountain Frontier," by Bakken, 855
 "The Development of the Family and Marriage in Europe," by Goody, 1050
 Dewald, Jonathan (R), 1332
 DeWindt, Anne Reiber (R), 112
 Dhanagare, D. N., "Peasant Movements in India, 1920–1950," 832
 di Gattinara, Arborio, 798
 di Lisio, Guglielmo Moffa, 1356
 Díaz-Briquets, Sergio, "The Health Revolution in Cuba," 565
 "Diccionario de las letras nicaragüenses," pt. 1, "Escritores de la época colonial y el siglo XIX," by Arellano, 1190
 Dickens, Roy S., Jr., editor, "Archaeology of Urban America: The Search for Pattern and Process" (E), 253
 Dickerman, Edmund H. (R), 1333
 "Dictatorships in East-Central Europe, 1918–1939," edited by Zarnowski (E), 1430
 Diefendorf, Barbara B., "Paris City Councillors in the Sixteenth Century: The Politics of Patrimony," 131
 Diehl, James M. (R), 472
 Dill, Alonzo Thomas, "Carter Braxton, Virginia Signer: A Conservative in Revolt," 846
 "Dimensions of Orthodox Judaism," edited by Bulka (E), 900
 Din, Gilbert C., and Abraham P. Nasatir, "The Imperial Osages: Spanish-Indian Diplomacy in the Mississippi Valley," 1149
 Dingman, Roger (R), 539
 Dinkin, Robert J., "Voting in Revolutionary America: A Study of Elections in the Original Thirteen States, 1776–1789," 197
 Dinnerstein, Leonard (R), 1185
 "The Diocese of Vic," by Freedman, 1319
 "The Diplomacy of Ideas," by Ninkovich, 883
 DIPLOMATIC HISTORY: Abadi, "Britain's Withdrawal from the Middle East, 1947–1971," 776; Alexander, "The Prelude to the Truman Doctrine," 479; Anderson, "Latvijas Vēsture, 1920–1940," 1358; Barker, "The British between the Superpowers, 1945–1950," 1331; Berman, "The Influence of the Carnegie, Ford, and Rockefeller Foundations on American Foreign Policy," 1184; Bosworth, "Italy and the Approach of the First World War," 801; Buschkühl, "Great Britain and the Holy See, 1746–1870," 1068; Carley, "Revolution and Intervention," 1337; Cervo, "O parlamento brasileiro e as relações exteriores, 1826–1889," 240; Chester, "United States Oil Policy and Diplomacy," 884; Cole, "Roosevelt and the Isolationists, 1932–45," 538; Combs, "American Diplomatic History," 234; Dallek, "The American Style of Foreign Policy," 541; Donnini, "L'Accordo italo-russo di Racconigi," 1357; Edwards, "Prime Ministers and Diplomats," 1141; Guinsburg, "The Pursuit of Isolationism in the United States Senate from Versailles to Pearl Harbor," 882; Hartmann, "Die Beziehungen Preussens zu Dänemark von 1688 bis 1789," 1348; Head, "America's China Sojourn," 539; Heller, "British Policy towards the Ottoman Empire, 1908–1914," 773; Hughes, "Emotion and High Politics," 407; Kaufman, "Trade and Aid," 550; Kedourie and Haim, editors, "Zionism and Arabism in Palestine and Israel," 497; Kraszewski, "Polityka wielkiej Brytanii wobec Niemiec w latach 1918–1925," 774; Linke, "Das zarische Russland und der Erster Weltkrieg," 814; Mayer, "Egypt and the Palestine Question, 1936–1945," 178; McIntire, "England against the Papacy, 1858–1861," 1068; McKay and Scott, "The Rise of the Great Powers, 1648–1815," 405; McTague, "British Policy in Palestine, 1917–1922," 1366; Ninkovich, "The Diplomacy of Ideas," 883; Palmer, "The Chancelleries of Europe," 1045;

- Petracchi, "La Russia rivoluzionaria nella politica italiana," 477; Postma, "Viglius van Aytta als humanist en diplomaat, 1507–1549," 1343; Ránki, "Economy and Foreign Policy," 1104; Shlaim, "The United States and the Berlin Blockade, 1948–1949," 549; Thomas, "The Guarantee of Belgian Independence and Neutrality in European Diplomacy, 1830's–1930's," 1342; Toby, "State and Diplomacy in Early Modern Japan," 1375; Treadway, "The Falcon and the Eagle," 480; Tucker, "Patterns in the Dust," 1180; Vucinich, editor, "At the Brink of War and Peace," 481; Wah, "The Defence of Malaysia and Singapore," 836; Yoshitsu, "Japan and the San Francisco Peace Settlement," 1377; Zapantis, "Greek-Soviet Relations, 1917–1941," 478
- "Disaffection and Decadence," by Hansen, 137
- "Discarding the Asylum," by Rooke and Schnell, 1415
- "Disputes and Settlements," edited by Bossy (E), 1425
- "The Divided Mind of Protestant America, 1880–1930," by Szasz, 861
- Divine, Father (George Baker), 542
- "Divisions throughout the Whole," by Nobles, 1146
- Dobbin, Christine, "Islamic Revivalism in a Changing Peasant Economy: Central Sumatra, 1784–1847," 189
- Dobkowski, Michael N., and Isidor Wallimann, editors, "Towards the Holocaust: The Social and Economic Collapse of the Weimar Republic" (E), 573
- Dobyns, Henry F., "Their Number Become Thinned: Native American Population Dynamics in Eastern North America," 1380
- The Domestication of Politics: Women and American Political Society, 1780–1920*, by Baker, 620–47
- Donahoe, Bernard F. (R), 1405
- Donaldson, Gordon, 572
- Donnelly, James S., Jr., and Samuel Clark, editors, "Irish Peasants: Violence and Political Unrest, 1780–1914" (E), 897
- Donnini, Guido, "L'Accordo italo-russo di Racconigi," 1357
- "Dori," by Handler, 1111
- Dostian, I. S., *et al.*, editors, "Pervoe serbskoe vosstanie 1804–1813 gg. i Rossiia," vol. 2, "1808–1813," 1359
- Dostoevsky, Feodor Mikhailovich, 169
- "Dostoevsky, Grigor'ev, and Native Soil Conservatism," by Dowler, 169
- Doughty, Robin W., "Wildlife and Man in Texas: Environmental Change and Conservation," 1180
- Douglas, Roy, editor, "1939: A Retrospect Forty Years After" (E), 895
- Douglass, A. E., 534
- Dowler, Wayne, "Dostoevsky, Grigor'ev, and Native Soil Conservatism," 169
- Dozier, Robert R., "For King, Constitution, and Country: The English Loyalists and the French Revolution," 1328
- Drabek, Anna M., *et al.*, editors, "Ungarn und Österreich unter Maria Theresia und Joseph II: Neue Aspekte im Verhältnis der beiden Länder" (E), 250
- Dragnich, Alex N., "The First Yugoslavia: Search for a Viable Political System," 480
- Drake, Michael (R), 1096
- Drake, Michael, editor, "Population Studies from Parish Registers: A Selection of Readings from *Local Population Studies*" (E), 572
- Dray, William H. (R), 1047
- "Dreaming the Rational City," by Boyer, 1176
- Dreyer, Edward L. (R), 1134
- Dreyfus, Francois-Georges, editor, "Réformisme et révisionnisme dans les socialismes allemand, autrichien et français" (E), 1428
- "Drift toward Dissolution," by Freehling, 518
- Drinkwater, J. F., "Roman Gaul: The Three Provinces, 58 BC–AD 260," 1058
- Drummond, Ian M. (R), 771
- Duchhardt, Heinz, "Herrscherweihe und Königskrönung im frühneuzeitlichen Europa" (E), 1196
- Ducker, James H., "Men of the Steel Rails: Workers on the Atchison, Topeka, and Santa Fe Railroad, 1869–1900," 528
- Dudden, Faye E., "Serving Women: Household Service in Nineteenth-Century America," 206
- "The Dukeries Transformed," by Waller, 1330
- Dull, Jonathan R. (R), 1147
- Dulles, John W. F., "Brazilian Communism, 1935–1945: Repression during World Upheaval," 1421
- Dunbar, Robert G., "Forging New Rights in Western Waters," 838
- Duniway, Abigail Scott, 860
- Dunlay, Thomas W., "Wolves for the Blue Soldiers: Indian Scouts and Auxiliaries with the United States Army, 1860–90," 208
- Dunn, Dennis J. (R), 817
- Dunn, Richard S. (R), 841
- Duplessis, Robert S. (R), 1091
- Duram, James C. (R), 551
- Durkacz, Victor Edward, "The Decline of the Celtic Languages: A Study of Linguistic and Cultural Conflict in Scotland, Wales, and Ireland from the Reformation to the Twentieth Century," 127
- Dussel, Enrique D., "Historia general de la Iglesia en America latina," vol. 1, pt. 1, "Introducción general a la historia de la Iglesia en America latina," 1416
- "Dutch Authors on West Indian History," edited by Meilink-Roelofs (E), 254
- "Dutch Capitalism and World Capitalism," edited by Aymard, 789
- "Dutch Puritanism," by Sprunger, 788
- Duus, Peter (R), 827
- "The Dynamics of Agricultural Change," by Grigg, 1051
- Dyson, B. Patricia (R), 546
- Eadie, John W. (R), 745
- "Eagle against Empire," edited by Jeffreys-Jones (E), 1431
- Eagles, Charles W., "Jonathan Daniels and Race Relations: The Evolution of a Southern Liberal," 224
- Eakins, Thomas, 1398
- "The Earliest Wheeled Transport," by Piggott, 738
- "Early Adventist Educators," edited by Knight (E), 1202
- "Early American Taverns," by Rice, 1145
- Easley, Brian (R), 401
- "East Los Angeles," by Romo, 544
- Eastman, Charles, 858
- Eberhard, Winfried, "Konfessionsbildung und Stände in Böhmen, 1478–1530," 1102
- Eblen, Jack Ericson (R), 843
- Echard, William E., "Napoleon III and the Concert of Europe," 453
- Eckhardt, Celia Morris, "Fanny Wright: Rebel in America," 1393
- "The Eclipse of Darwinism," by Bowler, 402
- "Economic Development in the Habsburg Monarchy

- in the Nineteenth Century," edited by Komlos (E), 250
- ECONOMIC HISTORY: Alter, "Wissenschaft, Staat, Mäzene," 123; Anisimov, "Podatnaia reforma Petra I," 1117; Balfour, "West Germany," 156; Benigno, "Il porto di Trapani nel Settecento," 1354; Bergad, "Coffee and the Growth of Agrarian Capitalism in Nineteenth-Century Puerto Rico," 888; Borchardt, "Wachstum, Krisen, Handlungsspielräume der Wirtschaftspolitik," 465; Byrnes, editor, "After Brezhnev," 496; Carlton, "Mill and Town in South Carolina, 1880–1920," 210; Checkland, "British Public Policy, 1776–1939," 437; Cole and Park, "Financial Development in Korea, 1945–1978," 502; D'Arms, "Commerce and Social Standing in Ancient Rome," 740; de la Roncière, "Prix et salaires à Florence au XIV^e siècle, 1280–1380," 418; De Maddalena, "Dalla città al borgo," 799; de Meere, "Economische ontwikkeling en levensstandaard in Nederland gedurende de eerste helft van de negentiende eeuw," 1344; Dobbin, "Islamic Revivalism in a Changing Peasant Economy," 189; Edelstein, "Overseas Investment in the Age of High Imperialism," 771; Feldman *et al.*, editors, "Die deutsche Inflation," 792; Greenow, "Credit and Socioeconomic Change in Colonial Mexico," 238; Hahn, "Wirtschaftliche Integration im 19. Jahrhundert," 146; Hanson, "Economy and Society in Baroque Portugal, 1668–1703," 459; Harna *et al.*, "Materiály k politickým, hospodárským a sociálnym dejinám Československa v letech 1929–1939," 482; Hodges and Whitehouse, "Mohammed, Charlemagne, and the Origins of Europe," 1061; Issawi, "The Economic History of Turkey, 1800–1914," 497; Kabisch, "Deutsches Kapital in den USA," 233; Kaufman, "Capitalism, Slavery, and Republican Values," 520; Kaufman, "Trade and Aid," 550; Komlos, "The Habsburg Monarchy as a Customs Union," 1103; Koval'chenko *et al.*, "Sotsial'no-ekonomicheskii stroi pomeschchich'ego khoziaistva Evropeiskoi Rossii v epokhu kapitalizma," 170; Kumar and Desai, editors, "The Cambridge Economic History of India," vol. 2, "c. 1757–c. 1970," 829; Landes, "Revolution in Time," 1052; Leonhard, "Die Seestadt Ancona im Spätmittelalter," 1323; Liss, "Atlantic Empires," 92; Loveday, "The Rise and Decline of the American Cut Nail Industry," 1399; Matthews *et al.*, "British Economic Growth, 1856–1973," 1077; McAlpin, "Subject to Famine," 1139; McClintock and Lowenthal, editors, "The Peruvian Experiment Reconsidered," 891; Moore, "Growth with Self-Management," 805; Morriss, "The Royal Dockyards during the Revolutionary and Napoleonic Wars," 1328; Palliser, "The Age of Elizabeth," 423; Philip, "Oil and Politics in Latin America," 1187; Platt, "Foreign Finance in Continental Europe and the United States, 1815–1870," 1302; Plessis, "La Banque de France et ses deux cents actionnaires sous le Second Empire," 134; Pollins, "Economic History of the Jews in England," 1078; Press, editor, "Städtewesen und Merkantilismus in Mitteleuropa," 1349; Ránki, "Economy and Foreign Policy," 1104; Rösch, "Venedig und das Reich," 115; Schaeper, "The French Council of Commerce, 1700–1715," 131; Spooner, "Risks at Sea," 1092; Struve, "Die Republik Texas, Bremen und das Hildesheimische," 1155; Thuillier, "La monnaie en France au début du XIX^e siècle," 1335; Treadgold, "The Byzantine State Finances in the Eighth and Ninth Centuries," 110; Veyrassat, "Négociants et fabricants dans l'industrie contonnière suisse, 1760–1840," 797; Walter, "Venezuela und Deutschland, 1815–1870," 889; Weaver, "Class, State, and Industrial Structure," 1186; Whitten, "The Emergence of Giant Enterprise, 1860–1914," 1400; Zachariadou, "Trade and Crusade," 1126
- "Economic History of the Jews in England," by Pollins, 1078
- "The Economic History of Turkey, 1800–1914," by Issawi, 497
- "An Economic History of Women in America," by Matthaëi, 215
- "Economic, Political, and Security Issues in Southeast Asia in the 1980s," edited by Scalapino and Wanandi (E), 577
- "Economics and World Power," edited by Becker and Wells (E), 1430
- "The Economics of Political Instability," by Nafziger, 180
- "Economische ontwikkeling en levensstandaard in Nederland gedurende de eerste helft van de negentiende eeuw," by de Meere, 1344
- "Economy and Foreign Policy," by Ránki, 1104
- "Economy and Society in Baroque Portugal, 1668–1703," by Hanson, 459
- Eddy, Samuel (R), 1312
- Edelstein, Michael, "Overseas Investment in the Age of High Imperialism: The United Kingdom, 1850–1914," 771
- Edmunds, R. David (R), 1149
- Edmunds, R. David, "The Shawnee Prophet," 848
- "Educating Black Doctors," by Summerville, 1166
- EDUCATION: Albisetti, "Secondary School Reform in Imperial Germany," 144; Anderson, "Education and Opportunity in Victorian Scotland," 1331; Axelrod, "Scholars and Dollars," 237; Blyth, "English University Adult Education, 1908–1958," 443; Borthwick, "Education and Social Change in China," 500; Byrnes, "Awakening American Education to the World," 544; Cohen, "A Calculating People," 203; Engel, "From Clergyman to Don," 442; Fuller, "The Old Country School," 531; Goodenow and Ravitch, editors, "Schools in Cities," 1176; Heyd, "Between Orthodoxy and the Enlightenment," 474; Härkönen, "Kouluylihallituksen ensimmäisen päällikön Casimir von Kothernin koulupoliitiikka," 1097; Metcalf, "From Little Rock to Boston," 551; Monaghan, "A Common Heritage," 202; O'Day, "Education and Society, 1500–1800," 116; Pichardo, "Biografía del Colegio de San Cristóbal de La Habana," 1422; Renger, "Die Gründung und Einrichtung der Universität Bonn und die Berufungspolitik des Kultusministers Altenstein," 143; Rubinger, "Private Academies of Tokugawa Japan," 1376; Saad, "Social History of Timbuktu," 1371; Simpson, "How the PhD Came to Britain," 1079; Stevens, "Law School," 1398; Summerville, "Educating Black Doctors," 1166; Tyack and Hansot, "Managers of Virtue," 531; Weisz, "The Emergence of Modern Universities in France, 1863–1914," 138
- "Education and Opportunity in Victorian Scotland," by Anderson, 1331
- "Education and Social Change in China," by Borthwick, 500
- "Education and Society, 1500–1800," by O'Day, 116
- "Edward F. Beale and the American West," by Thompson, 852
- "Edward III and Chivalry," by Vale, 749
- Edwards, Jerome E. (R), 226

- Edwards, Mark U., Jr., "Luther's Last Battles: Politics and Polemics, 1531-46," 463
- Edwards, P. G., "Prime Ministers and Diplomats: The Making of Australian Foreign Policy, 1901-1949," 1141
- "The Effects of World War I," vol. 13 of "War and Society in East Central Europe," edited by Banac (E), 575
- Egan, Clifford L., "Neither Peace Nor War: Franco-American Relations, 1803-1812," 198
- "Egypt and the Palestine Question, 1936-1945," by Mayer, 178
- "The Egypt of Nasser and Sadat," by Waterbury, 1127
- Ehrman, John, "The Younger Pitt: The Reluctant Transition," 766
- Eidlin, Fred (C), 1218
- "Eighteenth and Nineteenth Centuries," vol. 1 of "History of the Balkans," by Jelavich, 1116
- Eisen, Arnold M., "The Chosen People in America: A Study in Jewish Religious Ideology," 1178
- "Die Eisen- und Stahlindustrie des Ruhrgebiets, 1879-1914," by Feldenkirchen, 147
- Eisenhower, Dwight D., 550
- "Eisphora-Syntaxis-Stratitotika," by Brun, 1312
- Ekirch, A. Roger, *Great Britain's Secret Convict Trade to America, 1783-1784* (Research Note), 1285-91
- Eksteins, Modris (R), 466
- Eldot, Paula, "Governor Alfred E. Smith: The Politician as Reformer," 867
- Eliav-Feldon, Miriam, "Realistic Utopias: The Ideal Imaginary Societies of the Renaissance, 1516-1630," 91
- ELITES: Amelang, *Barristers and Judges in Early Modern Barcelona: The Rise of a Legal Elite*, 1264-84; Banac and Bushkovitch, editors, "The Nobility in Russia and Eastern Europe," 807; Bush, "The European Nobility," vol. 1, "Noble Privilege," 1293; Cardoza, "Agrarian Elites and Italian Fascism," 160; Chaline, "Les bourgeois de Rouen," 134; Crummey, "Aristocrats and Servitors," 808; Dardess, "Confucianism and Autocracy," 1134; Diefendorf, "Paris City Councillors in the Sixteenth Century," 131; Estèbe, "Les ministres de la République, 1871-1914," 138; Frame, "English Lordship in Ireland, 1318-1361," 111; Hopkins, "Death and Renewal," 741; Ives, "The Common Lawyers of Pre-Reformation England," 756; Jenkins, "The Making of a Ruling Class," 765; Lukacs, "Philadelphia," 878; Stafford, "Queens, Concubines, and Dowagers," 106; Tjerneld, "Fråan borgarståndets storhetstid," 1095; Tomás y Valiente, "Los validos en la monarquía española del siglo XVII," 786
- Elizabeth I (Tudor), 424
- Ellersieck, Heinz E. (R), 1095
- Elliott, Marianne, "Partners in Revolution: The United Irishmen and France," 129
- Ellis, Joseph J. (R), 515
- Ellis, Richard N. (R), 208
- Ellison, Herbert J. (R), 494
- Ellsworth, Scott, "Death in a Promised Land: The Tulsa Race Riot of 1921," 859
- Elton, G. R., "Studies in Tudor and Stuart Politics and Government," vol. 3, "Papers and Reviews, 1973-1981" (E), 896
- Elwitt, Sanford (R), 138
- Embry, Jessie L., and Thomas G. Alexander, editors, "After 150 Years: The Latter-day Saints in Sesquicentennial Perspective" (E), 252
- Emel'ianova, I. A., "'Vseobshchaia istoriia prava' v russkom dorevoliutsionnom pravovedenii, XIX vek," 488
- "The Emergence of Giant Enterprise, 1860-1914," by Whitten, 1400
- "The Emergence of Modern South Africa," by Yudelman, 181
- "The Emergence of Modern Universities in France, 1863-1914," by Weisz, 138
- Emerson, Roger L., *et al.*, editors, "Man and Nature" (E), 570
- "Eminent Elizabethans," by Rowse, 424
- Emmett, Alanna M., and Brian Croke, editors, "History and Historians in Late Antiquity" (E), 1426
- Emmons, Terence (R), 1118
- Emmons, Terence, "The Formation of Political Parties and the First National Elections in Russia," 812
- "Emotion and High Politics," by Hughes, 407
- "The Emperor and His Chancellor," by Headley, 798
- "The Empire of the Mother," by Ryan, 205
- "Empirische Sozialforschung," by Kern, 142
- "The End of the Russian Land Commune, 1905-1930," by Atkinson, 1119
- ENERGY STUDIES: Adams, "Paradoxical Harvest," 772
- Engel, A. J., "From Clergyman to Don: The Rise of the Academic Profession in Nineteenth-Century Oxford," 442
- Engel, Arthur (R), 1331
- Engel, Barbara Alpern (R), 1048
- Engel, Barbara Alpern, "Mothers and Daughters: Women of the Intelligentsia in Nineteenth-Century Russia," 809
- Engel, J. Ronald, "Sacred Sands: The Struggle for Community in the Indiana Dunes," 535
- Engelstein, Laura, "Moscow, 1905: Working-Class Organization and Political Conflict," 490
- Engerman, Stanley L. (R), 512
- "England against the Papacy, 1858-1861," by McIntire, 1068
- England, J. Merton, "A Patron for Pure Science: The National Science Foundation's Formative Years, 1945-57," 102
- England, John, 851
- Englander, David, "Landlord and Tenant in Urban Britain, 1838-1918," 769
- "Der englische Weg zum Sozialismus," by Wittig, 770
- English, John (R), 559
- "English Lordship in Ireland, 1318-1361," by Frame, 111
- "The English Rothschilds," by Davis, 1077
- "The English Seaside Resort," by Walton, 443
- "The English Terraced House," by Muthesius, 768
- "English University Adult Education, 1908-1958," by Blyth, 443
- Engman, Max, "St. Petersburg och Finland: Migration och influens, 1703-1917," 1097
- "Die Entfaltung politischer Öffentlichkeit in Russland, 1906-1914," by Hagen, 491
- "Entrepreneurship in Imperial Russia and the Soviet Union," edited by Guroff and Carstensen (E), 1200
- "Die Entstehung der 'Bourses du travail,'" by Schöttler, 456
- Epstein, James (C), 1222
- Erasmus, 1098
- Erickson, Carolly, "The First Elizabeth," 424
- Erickson, John, "The Road to Berlin: Continuing the History of Stalin's War with Germany," 1122
- Ermarth, Michael (R), 1038
- "Ernst Kantorowicz und Stefan George," by Grünwald, 466
- "Escape from Empire," by Moore, 439

- "Les esclaves en Grèce ancienne," by Garlan, 104
 "Escritores de la época colonial y el siglo XIX," pt. 1 of "Diccionario de las letras nicaragüenses," by Arellano, 1190
 Esler, Anthony (R), 95
 Esposito, John L., editor, "Voices of Resurgent Islam" (E), 1201
 "Essays in the History of Canadian Law," vol. 2, edited by Flaherty (E), 578
 "Essays on Frontiers in World History," by Powell *et al.* (E), 894
 "Essays on Lincoln's Faith and Politics," edited by Thompson (E), 1432
 "Essays on the Family and Historical Change," by Levine *et al.* (E), 895
 "Essays on World War I," edited by Williamson and Pastor (E), 242
 Essig, James D., "The Bonds of Wickedness: American Evangelicals against Slavery, 1770–1808," 196
 Estèbe, Jean, "Les ministres de la République, 1871–1914," 138
 "Ethiopia, Great Britain, and the United States, 1941–1974," by Marcus, 1132
 "Ethnic and Immigration Groups," edited by Rosof *et al.* (E), 241
 "Ethnic Theatre in the United States," edited by Seller (E), 578
 "Ethnicity and Equality," by Katzenstein, 508
 ETHNICITY AND ETHNIC HISTORY: Chan, "Gold Mountain," 886; Katzenstein, "Ethnicity and Equality," 508; Mirak, "Torn between Two Lands," 1401; Murphy, "Guestworkers in the German Reich," 1351; Nelli, "From Immigrants to Ethnic," 543; Ryan, "Beyond the Ballot Box," 523; Sánchez Korrol, "From Colonia to Community," 878; Sawyer, "Kings and Vikings," 745; Siaw, "Chinese Society in Rural Malaysia," 1140; Suppan, "Die österreichischen Volksgruppen," 1104
 Etlin, Richard A., "The Architecture of Death: The Transformation of the Cemetery in Eighteenth-Century Paris," 1335
 "Études africaines: Offertes à Henri Brunschwig" (E), 251
 Etzold, Thomas H. (R), 540; 883
 "Europa nach dem Zweiten Weltkrieg, 1945–1982," edited by Benz and Graml (E), 1196
 "Europe and Scandinavia," edited by Rystad (E), 1196
 "Europe and the People without History," by Wolf, 89
 "Europe in 1830," by Church, 406
 "The European Nobility," vol. 1, "Noble Privilege," by Bush, 1293
 "The European Realist Tradition," edited by Weisberg, 94
 "The Evangelical Alliance for the United States of America, 1847–1900," by Jordan, 861
 Evans, George de Lacy, 1071
 "Eve and the New Jerusalem," by Taylor, 121
 Evergates, Theodore (R), 753
 "The Evolution of Management Education," by Sedlak and Williamson, 1409
 "Evolution of the European Idea, 1914–1932," by Pegg, 1303
The Evolution of White Women's Experience in Early America, by Norton, 593–619
 "Exile Politics during the Second World War," by Glees, 127
 "L'Expansion et l'impuissance, 1952–1958," vol. 2 of "La France de la IV^e République," by Rioux, 785
 "Expedition in East-Central Africa, 1888–1891," by Wiese, 1131
 "Explaining Hitler's Germany," by Hiden and Farquharson, 1102
 EXPLORATION AND COLONIZATION: Karamanski, "Fur Trade and Exploration" 556; Spate, "The Pacific since Magellan," vol. 2, "Monopolists and Freebooters," 734; Wiese, "Expedition in East-Central Africa, 1888–1891," 1131; Wood, *La Salle: Discovery of a Lost Explorer*, 294–323
 Expositio and Oblatio: *The Abandonment of Children and the Ancient and Medieval Family*, by Boswell, 10–33
 "The Extended Family," edited by Minault, 833
 Ezergailis, Andrew, "The Latvian Impact on the Bolshevik Revolution: The First Phase; September 1917 to April 1918," 1121
 Ezergailis, Andrew, and Gert Von Pistohlkors, editors, "The Russian Baltic Provinces between the 1905/1917 Revolutions" (E), 251
 "The Face of the Past," by Dellheim, 123
 "Facing the Enlightenment and Pietism," by Loetscher, 202
 Fagen, Richard R., and Olga Pellicer, editors, "The Future of Central America: Policy Choices for the U.S. and Mexico" (E), 901
 "Faint Praise," by Baily, 869
 Fairbank, John K., editor, "The Cambridge History of China," vol. 12, pt. 1, "Republican China, 1912–1949," 1133
 "Faith by Statute," by Jones, 1325
 "The Falcon and the Eagle," by Treadway, 480
 "Family Forms in Historic Europe," edited by Wall *et al.* (E), 244
 FAMILY HISTORY: Baker, "The Percys of Mississippi," 1171; Boswell, *Expositio and Oblatio: The Abandonment of Children and the Ancient and Medieval Family*, 10–33; Collomp, "La maison du père," 1334; Davis, "The English Rothschilds," 1077; Fuchs, "Abandoned Children," 1336; Goody, "The Development of the Family and Marriage in Europe," 1050; Lewis, "The Pursuit of Happiness," 1148; Mintz, "A Prison of Expectations," 431; Ozment, "When Fathers Ruled," 1295; Peterson, *No Angels in the House: The Victorian Myth and the Paget Women*, 677–708; Sommerville, "The Rise and Fall of Childhood," 95; Ward and Ward, *Infant Birth Weight and Nutrition in Industrializing Montreal*, 324–45
 "Fanny Wright," by Eckhardt, 1393
 Fanon, Frantz, 1129
 "Far China Station," by Johnson, 517
 "Farewell to the Party of Lincoln," by Weiss, 1404
 Farnsworth, Beatrice (R), 809
 Farquharson, John, and John Hiden, "Explaining Hitler's Germany: Historians and the Third Reich," 1102
 FASCISM: Anderson, "Fascists, Communists, and the National Government," 447; Cardoza, "Agrarian Elites and Italian Fascism," 160; Fischer, "Stormtroopers," 470; Fleming, "Hitler und die Endlösung," 152; Grill, "The Nazi Movement in Baden, 1920–1945," 471; Kater, "The Nazi Party," 469; Petzold, "Die Demagogie des Hitlerfaschismus," 151
 "The Fascist Challenge and the Policy of Appeasement," edited by Mommsen and Kettenacker (E), 568
 "Fascists, Communists, and the National Government," by Anderson, 447
 "Father Divine and the Struggle for Racial Equality," by Weisbrot, 542

- "Federico Barbarossa nel dibattito storiografico in Italia e in Germania," edited by Manselli and Riedmann, 114
- Féher, F., and R. F. Miller, editors, "Krushchev and the Communist World" (E), 1429
- Fehrenbacher, Don E. (R), 518
- Feigon, Lee, "Chen Duxiu: Founder of the Chinese Communist Party," 1375
- Feinberg, Richard E., editor, "Central America: International Dimensions of the Crisis" (E), 254
- Feingold, Henry L. (R), 872
- Feinman, Ronald L. (R), 1171
- Feldbæk, Ole, "Danmarks historie," vol. 4, "Tiden 1730–1814," 461
- Feldenkirchen, Wilfried, "Die Eisen- und Stahlindustrie des Ruhrgebiets, 1879–1914: Wachstum, Finanzierung, und Struktur ihrer Grossunternehmen," 147
- Feldman, Gerald D., *et al.*, editors, "Die deutsche Inflation: Eine Zwischenbilanz," 792
- Felix, David, "Marx as Politician," 1304
- Fell, A. London (R), 1040
- Fell, A. London, "Origins of Legislative Sovereignty and the Legislative State," vol. 1, "Corasius and the Renaissance Systematization of Roman Law"; vol. 2, "Classical, Medieval, and Renaissance Foundations of Corasius' Systematic Methodology," 1296
- Fennell, John, "The Crisis of Medieval Russia, 1200–1304," 487
- Fenske, Lutz, *et al.*, editors, "Institutionen, Kultur und Gesellschaft im Mittelalter: Festschrift für Josef Fleckenstein zu seinem 65. Geburtstag" (E), 1427
- Ferenczi, Caspar, "Aussenpolitik und Öffentlichkeit in Russland, 1906–1912," 491
- Ferling, John E. (R), 513
- Fernández, Mercedes Borrero, "El mundo rural sevillano en el siglo XV: Aljarafe y Ribera," 1088
- Fernández-Santamaría, J. A., "Reason of State and Statecraft in Spanish Political Thought, 1595–1640," 457
- Ferrill, Arther (R), 414
- "The Fertility Transition in Sweden," by Lockridge, 1096
- "Festivals and Ceremonies of the Roman Republic," by Scullard, 739
- Fetter, Bruce, "Colonial Rule and Regional Imbalance in Central Africa," 821
- "Feudal Assessments and the Political Community under Henry II and His Sons," by Keefe, 1320
- "Fevral'skaia burzhuazno-demokraticheskaia revoliutsiia 1917 g. v Rossii," by Pushkareva, 173
- Fevel, Gilles, "La 'Gazette' en province à travers ses réimpressions, 1631–1752: Une recherche analytique de la diffusion d'un ancien périodique dans toute la France," 779
- Field, Frank (R), 446
- Field, Phyllis F. (R), 1391
- "The Fight to Save the Redwoods," by Schrepfer, 534
- "File din istoria militară a poporului Român," edited by Ceausescu (E), 576
- "Film and Radio Propaganda in World War II," edited by Short (E), 568
- FILM HISTORY: Welch, "Propaganda and the German Cinema, 1933–1945," 466
- "Financial Development in Korea, 1945–1978," by Cole and Park, 502
- Fine, John V. A., "The Ancient Greeks: A Critical History," 1312
- Fingerhut, Eugene R., "Survivor: Cadwallader Colden II in Revolutionary America," 1388
- Fink, Gary M. (R), 227
- Fink, Leon, "Workingmen's Democracy: The Knights of Labor and American Politics," 864
- Finlayson, Michael G., "Historians, Puritanism, and the English Revolution: The Religious Factor in English Politics before and after the Interregnum," 1066
- Finzsch, Norbert, "Die Goldgräber Kaliforniens: Arbeitsbedingungen, Lebensstandard und politisches System um die Mitte des 19. Jahrhunderts," 206
- Fireside, Harvey (R), 174
- "The First Amendment Reconsidered," edited by Chamberlin and Brown (E), 253
- "The First Elizabeth," by Erickson, 424
- "First Rival of the Metropolitan Opera," by Cone, 1162
- "The First Serbian Uprising, 1804–1813," edited by Vucinich, 486
- "The First Yugoslavia," by Dragnich, 480
- Fischer, Conan, "Stormtroopers: A Social, Economic, and Ideological Analysis, 1929–35," 470
- Fischer, Klaus, "Galileo Galilei," 401
- Fischer, Wolfdietrich, and Jürgen Schneider, editors, "Das Heilige Land im Mittelalter: Begegnungsraum zwischen Orient und Okzident" (E), 244
- Fischer-Galati, Stephen (R), 116
- Fishburn, Janet F. (R), 862
- Fisher, Alan W. (R), 818
- Fisher, Michael H. (R), 188
- "Fisher Row," by Prior, 429
- Fitz, Jenő, "L'Administration des provinces pannoniennes sous le Bas-Empire romain," 745
- Fitzpatrick, Brian, "Catholic Royalism in the Department of the Gard, 1814–1852," 783
- Fitzpatrick, Sheila (R), 495
- Fitzsimons, M. A. (R), 1331
- Flader, Susan L., editor, "The Great Lakes Forest: An Environmental and Social History," 535
- Flaherty, David H., editor, "Essays in the History of Canadian Law," vol. 2 (E), 578
- Fleckenstein, Josef, 1427
- Fleming, Gerald, "Hitler und die Endlösung," 152
- Fleming, William J. (R), 1421
- Flick, Carlos (R), 432
- Flink, James J. (R), 1172
- Flint, John (R), 180
- Floristán Imízcoz, Alfredo, "La Merindad de Estella en la edad moderna: Los hombres y la tierra," 1088
- "Flowers in Salt," by Sievers, 1137
- "Flüchtlinge und Flüchtlingspolitik in Bayern, 1945–1950," by Bauer, 154
- Flynn, Charles L., Jr., "White Land, Black Labor: Caste and Class in Late Nineteenth-Century Georgia," 1161
- Foerster, Roland G., *et al.*, "Anfänge westdeutscher Sicherheitspolitik, 1945–1956," vol. 1, "Von der Kapitulation bis zum Plevén-Plan," 472
- Fogle, French R., and Louis A. Knafla, "Patronage in Late Renaissance England" (E), 572
- FOLKLORE: Schmitt, "The Holy Greyhound," 1063; Webster, "From Paracelsus to Newton," 401
- Foner, Eric, "Nothing But Freedom: Emancipation and Its Legacy," 1160
- "For King, Constitution, and Country," by Dozier, 1328
- Ford, Colin, and Brian Harrison, "A Hundred Years Ago: Britain in the 1880s in Words and Photographs," 434
- "Foreign Finance in Continental Europe and the United States, 1815–1870," by Platt, 1302

- "Forging New Rights in Western Waters," by Dunbar, 838
- "The Forgotten Victim," by Hartigan, 96
- "The Formation of Political Parties and the First National Elections in Russia," by Emmons, 812
- "The Formation of the Republican Party in New York," by Booraem, 1391
- Formisano, Ronald P., "The Transformation of Political Culture: Massachusetts Parties, 1790s–1840s," 200
- Fornara, Charles William, "The Nature of History in Ancient Greece and Rome," 1311
- Forster, Johann Reinhold, 120
- Forster, Robert (R), 450
- Fortescue, William, "Alphonse de Lamartine: A Political Biography," 1085
- Foster, Elizabeth Read, "The House of Lords, 1603–1649: Structure, Procedure, and the Nature of Its Business," 1067
- Foster, James C. (R), 229
- Foster, John E., editor, "The Developing West: Essays on Canadian History in Honor of Lewis H. Thomas" (E), 254
- Foucault, Michel, 90
- Foweraker, Joe, "The Struggle for Land: A Political Economy of the Pioneer Frontier in Brazil from 1930 to the Present Day," 1193
- Fox, Alistair, "Thomas More: History and Providence," 422
- Fox, Richard Wightman (R), 227
- Fox, Richard Wightman, and T. J. Jackson Lears, editors, "The Culture of Consumption: Critical Essays in American History, 1880–1980," 1172
- Fox, Stephen, "John Muir and His Legacy: The American Conservation Movement," 223
- Fox, Vivian C. (R), 431
- Foxe, John, 758
- "Fråan borgarståndets storhetstid," by Tjerneld, 1095
- Frame, Robin, "English Lordship in Ireland, 1318–1361," 111
- "La France de la IV^e République," vol. 2, "L'Expansion et l'impuissance, 1952–1958," by Rioux, 785
- Frandsen, Karl-Erik, "Vang og tægt: Studier over dyrkningssystemer og agrarstrukturer i Danmarks landsbyer, 1682–83," 1093
- Frankforter, A. Daniel (R), 749
- "The Frankish Kingdoms under the Carolingians, 751–987," by McKitterick, 1317
- Frantzen, Allen J., "The Literature of Penance in Anglo-Saxon England," 417
- Fraser, Derek, and Anthony Sutcliffe, editors, "The Pursuit of Urban History" (E), 568
- Fraser, Walter J., Jr., and Winfred B. Moore, Jr., editors, "The Southern Enigma: Essays on Race, Class, and Folk Culture" (E), 1201
- "Frauen suchen ihre Geschichte," edited by Hausen (E), 1199
- Frazee, Charles A., "Catholics and Sultans: The Church and the Ottoman Empire, 1453–1923," 1127
- Frederick I (Barbarossa), 114
- "Free Frank," by Walker, 1396
- Freedeman, Charles E. (R), 1335
- Freedman, Paul H., "The Diocese of Vic: Tradition and Regeneration in Medieval Catalonia," 1319
- Freehling, Alison Goodyear, "Drift toward Dissolution: The Virginia Slavery Debate of 1831–1832," 518
- Freeman, A. Z. (R), 749
- "French and Germans, Germans and French," by Cobb, 140
- "The French Colonial Lobby, 1889–1938," by Persell, 1086
- "French Communism in the Era of Stalin," by Wall, 1087
- "The French Council of Commerce, 1700–1715," by Schaeper, 131
- "The French New Left," by Hirsh, 141
- "French War Aims against Germany, 1914–1919," by Stevenson, 456
- Frey, Linda (R), 1348
- Friedel, Robert, "Pioneer Plastic: The Making and Selling of Celluloid," 1183
- Friedman, Ellen G., "Spanish Captives in North Africa in the Early Modern Age," 1129
- Friedman, Jerome, "The Most Ancient Testimony: Sixteenth-Century Christian-Hebraica in the Age of Renaissance Nostalgia," 1294
- Frier, Bruce W. (R), 1314
- Frijhoff, Willem, editor, "The Supply of Schooling" (E), 569
- Fritz, Martin, *et al.*, "The Adaptable Nation: Essays in Swedish Economy during the Second World War" (E), 249
- "From Alexander to Cleopatra," by Grant, 103
- "From Asylum to Welfare," by Simmons, 558
- "From Clergyman to Don," by Engel, 442
- "From Colonia to Community," by Sánchez Korrol, 878
- "From Hitler to Ulbricht," by Sandford, 473
- "From Home to Office," by Rotella, 1165
- "From Hungary to the United States, 1880–1914," by Puskás, 165
- "From Immigrants to Ethnic," by Nelli, 543
- From Learned Society to Professional Organization: The American Historical Association, 1884–1900*, by Van Tassel, 929–56
- "From Little Rock to Boston," by Metcalf, 551
- "From Paracelsus to Newton," by Webster, 401
- "From Peasant to Proletarian," by Goodman and Redclift, 1300
- "Frontier Regions in Western Europe," edited by Anderson (E), 244
- Fruin, W. Mark, "Kikkoman: Company, Clan, and Community," 1137
- Fuchs, Rachel Ginnis, "Abandoned Children: Foundlings and Child Welfare in Nineteenth-Century France," 1336
- Fuhrmann, Joseph T. (R), 1117
- Fuller, Wayne E., "The Old Country School: The Story of Rural Education in the Middle West," 531
- "Fur Trade and Exploration," by Karamanski, 556
- Furth, Charlotte (R), 1135
- "The Future of Central America," edited by Fagen and Pellicer (E), 901
- Gaddis, John Lewis, *Comment on Leffler, The American Conception of National Security and the Beginnings of the Cold War, 1945–48* (Forum), 382–85
- Gadsden, Christopher, 515
- Gagliardo, John G. (R), 1355
- Gainer, Bernard (R), 438
- Galasso, Giuseppe, and Carla Russo, editors, "Per la storia sociale e religiosa del Mezzogiorno d'Italia" (E), 248
- Galenson, David W. (R), 195
- Galenson, Walter, "The United Brotherhood of Carpenters: The First Hundred Years," 1400
- Galgano, Michael J. (R), 1326

- "Galicia," by Magocsi, 1115
 "Galileo Galilei," by Fischer, 401
 Gallet, Jean, "La seigneurie bretonne, 1450-1680: L'Exemple du Vannetais," 1332
 "A Gambling Style of Government," by Black, 509
 "The Games Must Go On," by Guttmann, 1309
 Gardinier, David E. (R), 1371
 Garfield, Robert (R), 1131
 Garland, Yvon, "Les esclaves en Grèce ancienne," 104
 Garthwaite, Gene R., "Khans and Shahs: A Documentary Analysis of the Bakhtiari in Iran," 1368
 "Gatekeepers of Black Culture," by Joyce, 211
 Gatewood, Willard B., Jr. (R), 218; 1369
 "Gauchos and the Vanishing Frontier," by Slatta, 1421
 "Le gaullisme d'opposition, 1946-1958," by Charlot, 784
 Gaustad, Edwin S. (R), 204
 Gay, Ebenezer, 1385
 "La 'Gazette' en province à travers ses réimpressions, 1631-1752," by Feyel, 779
 Geison, Gerald L., editor, "Professions and Professional Ideologies in America" (E), 1202
 Gelber, Harry G. (R), 1141
 Gelfand, Mark I. (R), 1175
 "'Gemeinwohl,' Polizei und 'Festungspraxis,'" by Lüdtkke, 146
 Gendzier, Irene L. (R), 1129
 GENEALOGY AND HERALDRY: Ladero Quesada, "El siglo XV en Castilla," 1088; Vale, "Edward III and Chivalry," 749; Vassberg, "La venta de tierras baldías," 1088
 "General John Burgoyne," by Hargrove, 428
 "General John M. Palmer, Citizen Soldiers, and the Army of a Democracy," by Holley, 225
 Genizi, Haim, "American Apathy: The Plight of Christian Refugees from Nazism," 1406
 "Le genti del mare Mediterraneo," edited by Ragosta (E), 248
 "Geology in the Nineteenth Century," by Greene, 101
 George, Stefan, 466
 Gerlich, Alois, editor, "Vom alten Reich zu neuer Staatlichkeit: Alzevier Kolloquium, 1979; Kontinuität und Wandel im Gefolge der französischen Revolution am Mittelrhein" (E), 247
 "German Workers in Industrial Chicago, 1850-1910," edited by Keil and Jentz, 1164
 "Germans in America," edited by McCormick (E), 900
 "Gerontologie und Sozialgeschichte," edited by Conrad and von Kondratowicz (E), 898
 Gerstner, Patsy A. (R), 101
The Gestalt Psychologists in Behaviorist America, by Sokal, 1240-63
 Getzler, Israel, "Kronstadt, 1917-1921: The Fate of a Soviet Democracy," 492
 "Die gewonnenen Jahre," by Imhof, 309
 Geyer, Michael (R), 148
 Gibson, Arrell Morgan, "The Santa Fe and Taos Colonies: Age of the Muses, 1900-1942," 536
 Gibson, Dale, *et al.*, "Attorney for the Frontier: Enos Stutsman," 1415
 "The Gift of Government," by Pole, 735
 Gilberg, Trond (R), 156
 Gilbert, Benjamin F. (R), 852
 Gilbert, Neal W. (R), 755
 Gillespie, Richard, "Soldiers of Perón: Argentina's Montoneros," 564
 Gillispie, Charles Coulston, "The Montgolfier Brothers and the Invention of Aviation, 1783-1784: With a Word on the Importance of Ballooning for the Science of Heat and the Art of Building Railroads," 1052
 Gilmore, William J. (R), 1389
 Ginsburg, Madeleine, "Victorian Dress in Photographs," 434
 "The Giriama and Colonial Resistance in Kenya, 1800-1920," by Brantley, 824
 "Giuseppe Volpi et l'Italie moderne," by Romano, 800
 Glees, Anthony, "Exile Politics during the Second World War: The German Social Democrats in Britain," 127
 Glen, Robert (R), 1075
 "God 1918i," by Mints, 1360
 Godbold, E. Stanly, Jr., and Robert H. Woody, "Christopher Gadsden and the American Revolution," 515
 Goffart, Walter (R), 110
 Goguel, Maurice, 1292
 Goitein, S. D., "A Mediterranean Society: The Jewish Communities of the Arab World As Portrayed in the Documents of the Cairo Geniza," vol. 4, "Daily Life," 1365
 "Gold Mountain," by Chan, 886
 "Gold Rush Sojourners in Great Salt Lake City, 1849 and 1850," by Madsen, 1393
 Goldberg, Hillel (R), 408
 Goldberg, Richard Thayer, "The Making of Franklin D. Roosevelt: Triumph over Disability," 867
 Goldfield, David R. (R), 880
 "Die Goldgräber Kaliforniens," by Finzsch, 206
 Goldstein, Robert J., "Political Repression in 19th-Century Europe," 1044
 Goldwert, Marvin (R), 564
 Goldwert, Marvin, "Psychic Conflict in Spanish America: Six Essays on the Psychohistory of the Region" (E), 254
 Gollin, Alfred (R), 125
 Gonzales, Michael J. (R), 890
 "Good and Faithful Labor," by Davis, 521
 Good, David F. (R), 233
 Goodenow, Ronald K., and Diane Ravitch, editors, "Schools in Cities: Consensus and Conflict in American Educational History," 1176
 Goodich, Michael (R), 1063
 Goodman, Allan E. (R), 554
 Goodman, David, and Michael Redclift, "From Peasant to Proletarian: Capitalist Development and Agrarian Transitions," 1300
 Goodman, Martin, "State and Society in Roman Galilee, A.D. 132-212," 1315
 Goodwin, Craufurd D. (R), 237
 Goody, Jack, "The Development of the Family and Marriage in Europe," 1050
 Gordon, Linda, "Cossack Rebellions: Social Turmoil in the Sixteenth-Century Ukraine," 168
 Gordon, Michael D. (R), 457
 Gorham, Deborah, "The Victorian Girl and the Feminine Ideal," 1329
 Gorny, Joseph, "The British Labour Movement and Zionism, 1917-1948," 441
 Gossman, Norbert J. (R), 430
 Gossweiler, Kurt, "Kapital, Reichswehr und NSDAP, 1919-1924," 148
 Gottfried, Robert S., "The Black Death: Natural and Human Disaster in Medieval Europe," 1322
 "Gottlieb Schober of Salem," by Surratt, 842
 Gouda, Frances (R), 787
 Gould, Lewis L. (R), 1403
 "The Government and Politics of Ireland," by Chubb, 777
 "Governor Alfred E. Smith," by Eldot, 867

- Grabbe, Hans-Jürgen, "Unionsparteien, Sozialdemokratie und Vereinigte Staaten von Amerika, 1945–1966," 155
- Graebner, William (R), 1400
- Graf, Christoph, "Politische Polizei zwischen Demokratie und Diktatur: Die Entwicklung der preussischen politischen Polizei vom Staatsschutzorgan der Weimarer Republik zum Geheimen Staatspolizeiamt des Dritten Reiches," 467
- Graff, Henry F., editor, "The Presidents: A Reference History" (E), 1431
- Graham, John T. (R), 137
- Graml, Hermann, and Wolfgang Benz, editors, "Europa nach dem Zweiten Weltkrieg, 1945–1982: Das Zwanzigste Jahrhundert II" (E), 1196
- Granatstein, J. L. (R), 886
- Grandin, Nicole, "Le Soudan nilotique et l'administration britannique, 1898–1956: Éléments d'interprétation sociohistorique d'une expérience coloniale," 124
- Grant, Michael, "From Alexander to Cleopatra: The Hellenistic World," 103
- Grantham, Dewey W., "Southern Progressivism: The Reconciliation of Progress and Tradition," 1403
- Graus, František, "Die Nationenbildung der Westslawen im Mittelalter," 419
- Grayson, Benson Lee, "United States–Iranian Relations," 885
- "A Great and Necessary Measure," by Bullion, 119
- "Great Britain and the Holy See, 1746–1870," by Buschkühl, 1068
- Great Britain's Secret Convict Trade to America, 1783–1784* (Research Note), by Ekirch, 1285–91
- "The Great Lakes Forest," edited by Flader, 535
- "The Great Leap Forward, 1958–1960," vol. 2 of "The Origins of the Cultural Revolution," by MacFarquhar, 1136
- Greaves, Richard (C), 590
- "Greek-Soviet Relations, 1917–1941," by Zapantis, 478
- Greenberg, Dolores (R), 101
- Greene, John C. (R), 402
- Greene, Mott T., "Geology in the Nineteenth Century: Changing Views of a Changing World," 101
- Greenleaf, Richard E. (R), 1416
- Greenough, Paul (R), 1139
- Greenow, Linda, "Credit and Socioeconomic Change in Colonial Mexico: Loans and Mortgages in Guadalajara, 1720–1820," 238
- Greenwald, Maurine Weiner (R), 193
- Gregg, Pauline, "King Charles I," 760
- Gregory, Derek, "Regional Transformation and Industrial Revolution: A Geography of the Yorkshire Woollen Industry," 1074
- Gregory, Timothy E. (R), 415
- Grendler, Paul F. (R), 1109
- Grenville, George, 119
- Gressley, Gene M. (R), 529
- "Griechische Töpferkunst," by Scheibler, 1311
- Grieder, Jerome B. (R), 1133
- Grierson, Philip, 569
- Griffin, Alan R. (R), 1330
- Griffith, Robert (R), 1413
- Griffiths, Gordon (R), 1343
- Griffiths, Ralph A. (R), 416
- Grigg, David, "The Dynamics of Agricultural Change: The Historical Experience," 1051
- Grigor'ev, Apollon, 169
- Grill, Johnpeter H. (R), 795
- Grill, Johnpeter Horst, "The Nazi Movement in Baden, 1920–1945," 471
- Grimmer, Claude, "Vivre à Aurillac au XVIII^{ème} siècle," 1083
- Grob, Gerald N., "Mental Illness and American Society, 1875–1940," 1167
- Grose, Peter, "Israel in the Mind of America," 1384
- Gross, Hanns (R), 477; 1347
- Gross, Nachum T. (R), 1103
- Grosseteste, Robert, 748
- "Growing Up in British India," by Walsh, 831
- "Growth with Self-Management," by Moore, 805
- Grübler, Michael, "Die Spitzenverbände der Wirtschaft und das erste Kabinett Brüning: Vom Ende der Grossen Koalition 1929/30 bis zum Vorabend der Bankenkrise 1931; Eine Quellenstudie," 149
- Grugel, Lee E. (R), 121
- Gruman, Gerald J. (R), 409
- "Die Gründung und Einrichtung der Universität Bonn und die Berufungspolitik des Kultusministers Altenstein," by Renger, 143
- Grünewald, Eckhart, "Ernst Kantorowicz und Stefan George: Beiträge zur Biographie des Historikers bis zum Jahre 1938 und zu seinem Jugendwerk *Kaiser Friedrich der Zweite*," 466
- "The Guarantee of Belgian Independence and Neutrality in European Diplomacy, 1830's–1930's," by Thomas, 1342
- "Guests in the Dragon," by Pasternak, 1374
- "Guestworkers in the German Reich," by Murphy, 1351
- Guggenheim, Scott E., and Robert P. Weller, editors, "Power and Protest in the Countryside: Studies of Rural Unrest in Asia, Europe, and Latin America" (E), 243
- "Guglielmo Moffa di Lisio, 1791–1877," by Nada, 1356
- Guha, Ranajit, editor, "Subaltern Studies," vol. 2, "Writings on South Asian History and Society" (E), 1430
- Guice, John D. W. (R), 855
- Guidi, José, *et al.*, "Le pouvoir et la plume: Incitation, contrôle et répression dans l'Italia du XVI^e siècle" (E), 574
- Guinebert, Charles, 1292
- Guilbaut, Serge, "How New York Stole the Idea of Modern Art: Abstract Expressionism, Freedom, and the Cold War," 1177
- Guinsburg, Thomas N., "The Pursuit of Isolationism in the United States Senate from Versailles to Pearl Harbor," 882
- Gullick, J. M. (R), 835
- Gunawardana, R. A. L. H., "Robe and Plough: Monasticism and Economic Interest in Early Medieval Sri Lanka," 188
- "Gunboats and Marines," by Cole, 538
- Gunn, Giles, editor, "The Bible and American Arts and Letters" (E), 1431
- Gunst, Péter, and Tamás Hoffmann, "Large Estates and Small Holdings in Europe in the Middle Ages and Modern Times: National Reports" (E), 243
- Guroff, Gregory, and Fred V. Carstensen, editors, "Entrepreneurship in Imperial Russia and the Soviet Union" (E), 1200
- "Gustav Adolf—der Grosse," by Barudio, 1095
- Guttmann, Allen (R), 1168
- Guttmann, Allen, "The Games Must Go On: Avery Brundage and the Olympic Movement," 1309
- Guzman, Gregory G. (R), 751

- "H. I. Kimball, Entrepreneur," by Reagan, 526
- Haber, Carole, "Beyond Sixty-Five: The Dilemma of Old Age in America's Past," 193
- Haber, Francis C. (R), 120
- Habib, Irfan (R), 829
- "The Habsburg Monarchy as a Customs Union," by Komlos, 1103
- "Haciendas and Economic Development," by Lindley, 1189
- Haddad, Yvonne Yazbeck, *et al.*, editors, "The Islamic Impact" (E), 1430
- Haesenne-Peremans, Nicole, "Les pauvres et le pouvoir: Assistance et répression au pays de Liège, 1685–1830," 1091
- Haffner, Sebastian, and Wolfgang Venohr, "Profils Prussiens" (E), 898
- Hagen, Manfred, "Die Entfaltung politischer Öffentlichkeit in Russland, 1906–1914," 491
- Hahn, Hans-Werner, "Wirtschaftliche Integration im 19. Jahrhundert: Die Hessischen Staaten und der Deutsche Zollverein," 146
- Hahn, Steven, "The Roots of Southern Populism: Yeoman Farmers and the Transformation of the Georgia Upcountry, 1850–1890," 854
- Haim, Sylvia G., and Elie Kedourie, editors, "Zionism and Arabism in Palestine and Israel," 497
- Hakuseki, Arai, "Tokushi Yoron," 186
- Hall, Grover C., 876
- Hall, Peter Dobkin (R), 1148
- Halstead, John P., "The Second British Empire: Trade, Philanthropy, and Good Government, 1820–1890," 1076
- Hamalainen, Pekka Kalevi (R), 158
- Hamerow, Theodore S. (R), 1099
- Hamerow, Theodore S., "The Birth of a New Europe: State and Society in the Nineteenth Century," 736
- Hamilton, David, "The Healers: A History of Medicine in Scotland," 128
- Hamilton, Richard F. (R), 469
- Hamilton, Roberta (R), 236
- Hammack, David C. (R), 878
- Hammack, David C., "Power and Society: Greater New York at the Turn of the Century," 213
- Hammett, Hugh B. (R), 853
- Hampson, Norman, "Will and Circumstance: Montesquieu, Rousseau, and the French Revolution," 1084
- Hanagan, Michael (R), 456
- Hanchett, William (R), 1395
- Hanchett, William, "The Lincoln Murder Conspiracies," 1395
- Handler, Andrew, "Dori: The Life and Times of Theodor Herzl in Budapest, 1860–1878," 1111
- Hanfmann, George M. A., *et al.*, "Sardis from Prehistoric to Roman Times: Results of the Archaeological Exploration of Sardis, 1958–1975," 739
- Hankinson, Alan, "Man of Wars: William Howard Russell of *The Times*," 767
- Hanley, Sarah (R), 130
- Hanley, Sarah, "The *Lit de Justice* of the Kings of France: Constitutional Ideology in Legend, Ritual, and Discourse," 1333
- Hannig, Jürgen, "Consensus fidelium: Frühfeudale Interpretationen des Verhältnisses von Königtum und Adel am Beispiel des Frankenreiches," 419
- Hansen, Eric C., "Disaffection and Decadence: A Crisis in French Intellectual Thought, 1848–1898," 137
- Hanson, Carl A., "Economy and Society in Baroque Portugal, 1668–1703," 459
- Hanson, William S., and Gordon S. Maxwell, "Rome's North West Frontier: The Antonine Wall," 743
- Hansot, Elisabeth, and David Tyack, "Managers of Virtue: Public School Leadership in America, 1820–1980," 531
- Harbaugh, William H. (R), 213
- "Hard Times in Oklahoma," edited by Hendrickson (E), 900
- Harder, Hans-Bernd, editor, "Landesbeschreibungen Mitteleuropas vom 15. bis 17. Jahrhundert: Vorträge der 2. internationalen Tagung des 'Slawenkomitees' im Herder-Institut Marburg a. d. Lahn, 10.–13. November 1980" (E), 1429
- Hargrove, Richard J., Jr., "General John Burgoyne," 428
- Harik, Iliya (R), 1127
- Härkönen, Mirja, "Kouluyhällituksen ensimmäisen päällikön Casimir von Kothenin koulupolitiikka: Taustaa-tavoitteita-tuloksia," 1097
- Harlan, Louis R., "Booker T. Washington: The Wizard of Tuskegee, 1901–1915," 218
- "The Harmonization of European Public Policy," edited by Hurwitz (E), 896
- Harms, Robert (R), 1130
- Harna, Josef, *et al.*, "Materiály k politickým, hospodářským a sociálním dějinám Československa v letech 1929–1939," 482
- Harries, Karsten, "The Bavarian Rococo Church: Between Faith and Aestheticism," 464
- Harring, Sidney L., "Policing a Class Society: The Experience of American Cities, 1865–1915," 1401
- Harriot, Thomas, 1069
- Harris, Barbara J. (R), 1329
- Harris, J. R. (R), 772
- Harris, R. W., "Clarendon and the English Revolution," 1326
- Harrison, Brian (R), 837
- Harrison, Brian, "Peaceable Kingdom: Stability and Change in Modern Britain" (E), 245
- Harrison, Brian, and Colin Ford, "A Hundred Years Ago: Britain in the 1880s in Words and Photographs," 434
- Harte, N. B., and K. G. Ponting, editors, "Cloth and Clothing in Medieval Europe: Essays in Memory of Professor E. M. Carus-Wilson" (E), 1427
- Hartigan, Richard Shelly, "The Forgotten Victim: A History of the Civilian," 96
- Hartmann, Stefan, "Die Beziehungen Preussens zu Dänemark von 1688 bis 1789," 1348
- Hartog, Hendrik, "Public Property and Private Power: The Corporation of the City of New York, 1730–1870," 1149
- Hartung, Wolfgang, "Süddeutschland in der frühen Merowingerzeit: Studien zu Gesellschaft, Herrschaft, Stammesbildung bei Alamannen und Bajuwaren," 1064
- Hasegawa, Tsuyoshi (R), 173
- Haskell, Thomas L., editor, "The Authority of Experts: Studies in History and Theory" (E), 1425
- Hassler, Warren W., Jr. (R), 1144
- Hattaway, Herman, and Archer Jones, "How the North Won: A Military History of the Civil War," 207
- Hatzenbuehler, Ronald L., and Robert L. Ivie, "Congress Declares War: Rhetoric, Leadership, and Partisanship in the Early Republic," 1391
- Hauptman, Laurence M. (R), 870
- Hausen, Karin, editor, "Frauen suchen ihre Geschichte: Historische Studien zum 19. und 20. Jahrhundert" (E), 1199
- Hawley, Ellis W. (R), 545
- Hayden, J. Michael (R), 1333

- Hayman, Robert W., "Catholicism in Rhode Island and the Diocese of Providence, 1780–1886," 1151
- Head, William P., "America's China Sojourn: America's Foreign Policy and Its Effects on Sino-American Relations, 1942–1948," 539
- Headley, John M., "The Emperor and His Chancellor: A Study of the Imperial Chancellery under Gattinara," 798
- Headrick, Daniel R. (R), 1090
- "The Healers," by Hamilton, 128
- "The Health Revolution in Cuba," by Díaz-Briquets, 565
- HEALTH, DISEASE, AND NUTRITION: Spree, "Soziale Ungleichheit vor Krankheit und Tod," 790; Ward and Ward, *Infant Birth Weight and Nutrition in Industrializing Montreal*, 324–45
- Healy, David (R), 882
- Hearden, Patrick J., "Independence and Empire: The New South's Cotton Mill Campaign, 1865–1901," 211
- Heath, Jim F. (R), 1414
- Hechter, Michael, editor, "The Microfoundations of Macrosociology" (E), 571
- Heckart, Beverly (R), 144; 1302
- "Das Heilige Land im Mittelalter," edited by Fischer and Schneider (E), 244
- Heinemann, Ronald L., "Depression and New Deal in Virginia: The Enduring Dominion," 868
- Heinemann, Ulrich, "Die verdrängte Niederlage: Politische Öffentlichkeit und Kriegsschuldfrage in der Weimarer Republik," 1352
- Heinig, Paul-Joachim, "Reichsstädte, freie Städte und Königtum, 1389–1450: Ein Beitrag zur deutschen Verfassungsgeschichte," 1345
- Held, Joseph (R), 482
- Held, Joseph, editor, "The Cult of Power: Dictators in the Twentieth Century" (E), 895
- Heller, Agnes, editor, "Lukács Reappraised" (E), 1196
- Heller, Joseph, "British Policy towards the Ottoman Empire, 1908–1914," 773
- Hellie, Richard (R), 411
- Hellman, John (R), 140
- Hellman, John, "Simone Weil: An Introduction to Her Thought," 446
- Helms, Christine Moss (R), 1368
- Helms, Christine Moss, "The Cohesion of Saudi Arabia: Evolution of Political Identity," 819
- "Hemmakriget," by Molin, 156
- Henderson, W. O. (R), 146; 1078
- Hendrickson, Kenneth E., Jr., editor, "Hard Times in Oklahoma: The Depression Years" (E), 900
- Henneman, John Bell (R), 1324
- Henriksson, Anders, "The Tsar's Loyal Germans: The Riga Community; Social Change and the Nationality Question, 1855–1905," 170
- Henry II, 1320
- "Henry Stubbe, Radical Protestantism, and the Early Enlightenment," by Jacob, 426
- "Hensley Henson," by Chadwick, 1080
- Henson, Hensley, 1080
- Hepburn, A. C. (R), 777
- Herbert Baxter Adams and the Study of Local History*, by Higham, 1225–39
- Herbert Hoover and Conservation, 1921–33*, by Clements, 67–88
- Herde, Peter, "Pearl Harbor, 7. Dezember 1941: Der Ausbruch des Krieges zwischen Japan und den Vereinigten Staaten und die Ausweitung des europäischen Krieges zum Zweiten Weltkrieg," 540
- "The Heritage of Arung Palakka," by Andaya, 510
- Hernández Esteve, Esteban, "Creación del consejo de hacienda de Castilla, 1523–1525," 1341
- Herring, George C. (R), 1411
- "Herrscherweihe und Königskrönung im frühneuzeitlichen Europa," by Duchhardt (E), 1196
- Hertner, Peter, and Giorgio Mori, editors, "La transizione dall'economia di guerra all'economia di pace in Italia e in Germania dopo la Prima guerra mondiale" (E), 575
- Herzig, Arno, *et al.*, editors, "Arbeiter in Hamburg: Unterschichten, Arbeiter und Arbeiterbewegung seit dem ausgehenden 18. Jahrhundert" (E), 572
- Herzl, Theodor, 1111
- Herzstein, Robert Edwin (R), 793
- Hess, Andrew C. (R), 1129
- Heuman, Gad (R), 561
- Heussler, Robert, "Completing a Stewardship: The Malayan Civil Service, 1942–1957," 1379
- Heyd, Michael, "Between Orthodoxy and the Enlightenment: Jean-Robert Chouet and the Introduction of Cartesian Science in the Academy of Geneva," 474
- Hibbard, Caroline M., "Charles I and the Popish Plot," 760
- "The Hidden Sun," by Robins-Mowry, 501
- Hiden, John, and John Farquharson, "Explaining Hitler's Germany: Historians and the Third Reich," 1102
- Hietanen, Silvo, "Siirtoväen Pika-Asutuslaki 1940: Asutuspoliittinen tausta ja sisältö sekä toimeenpano," 158
- "High and Low Politics in Modern Britain," edited by Bentley and Stevenson (E), 896
- Higham, John, *Herbert Baxter Adams and the Study of Local History*, 1225–39
- Higonnet, Patrice (R), 1083
- Hill, Christopher, *et al.*, "The World of the Muggletons," 426
- Hill, Leonidas E., editor, "Die Weizsäcker-Papiere, 1900–1932," 150
- Hill, Marvin S. (R), 851
- Hill, Mary A. (R), 860
- Hilton, R. H., 241
- Himka, John-Paul, "Socialism in Galicia: The Emergence of Polish Social Democracy and Ukrainian Radicalism, 1860–1890," 1114
- Himmelfarb, Gertrude, "The Idea of Poverty: England in the Early Industrial Age," 1073
- Hinojosa, Gilberto Miguel, "A Borderlands Town in Transition: Laredo, 1755–1870," 1150
- Hinton, Harold C. (R), 1136
- Hirsh, Arthur, "The French New Left: An Intellectual History from Sartre to Gorz," 141
- "Hispano-Jewish Culture in Transition," by Septimus, 420
- "Histoire de l'administration de l'enseignement en France, 1789–1981," by Bousquet *et al.* (E), 246
- "Historia general de la Iglesia en America latina," vol. 1, pt. 1, "Introducción general a la historia de la Iglesia en America latina," by Dussel, 1416
- "Historians and the American West," edited by Malone (E), 578
- "Historians, Puritanism, and the English Revolution," by Finlayson, 1066
- Historical Consciousness in Nineteenth-Century America*, by Ross, 909–28
- "Historical Sociology," by Abrams, 1298
- "Historiography," by Breisach, 1038
- HISTORIOGRAPHY AND HISTORICAL METHODS: Adams, "Paradoxical Harvest," 772; Basarab, "Pereiaslav 1654," 808; Björk, "Den historiska argumenteringen," 1094; Breisach, "Historiography," 1038; Brenner, "History—The Human Gamble," 1299; Cherepnin, "Voprosy

- metodologii istoricheskogo issledovaniia," 486;
 Christ, "Römische Geschichte und deutsche Geschichtswissenschaft," 105; Curtin, *Depth, Span, and Relevance*, 1–9; Dussel, "Historia general de la Iglesia en America latina," vol. 1, pt. 1, "Introducción general a la historia de la Iglesia en America latina," 1416; Fornara, "The Nature of History in Ancient Greece and Rome," 1311;
 Gordon, "Cossack Rebellions," 168; Grigg, "The Dynamics of Agricultural Change," 1051;
 Hakuseki, "Tokushi Yoron," 186; Hidden and Farquharson, "Explaining Hitler's Germany," 1102;
 Higham, *Herbert Baxter Adams and the Study of Local History*, 1225–39; Hromnik, "Indo-Africa," 822;
 Meier and Rudwick, *J. Franklin Jameson, Carter G. Woodson, and the Foundations of Black Historiography* (Research Note), 1005–15; Norton, *The Evolution of White Women's Experience in Early America*, 593–619;
 Ross, *Historical Consciousness in Nineteenth-Century America*, 909–28; Singal, *Beyond Consensus: Richard Hofstadter and American Historiography*, 976–1004;
 Smith, *The Contribution of Women to Modern Historiography in Great Britain, France, and the United States, 1750–1940*, 709–32; Van Seters, "In Search of History," 412; Vasquez, "The Power of Power Politics," 1046; Wolf, "Europe and the People without History," 89
 "Den historiska argumenteringen," by Björk, 1094
 "History and Historians in Late Antiquity," edited by Croke and Emmett (E), 1426
 "History of Agriculture in Ohio to 1880," by Jones, 1161
 "History of Bourgeois Perception," by Lowe, 1301
 "A History of Cambodia," by Chandler, 834
 "A History of European Socialism," by Lindemann, 92
 "The History of Ideas," edited by King (E), 1425
 "A History of Rice University," by Meiners, 881
 "A History of Tasmania," vol. 1, "Van Diemen's Land from the Earliest Times to 1855," by Robson, 1143
 "History of the Balkans," vol. 1, "Eighteenth and Nineteenth Centuries"; vol. 2, "Twentieth Century," by Jelavich, 1116
 "History of the General Federation of Trade Unions, 1899–1980," by Prochaska, 125
 "History of the Militia and the National Guard," by Mahon, 1144
 "History—The Human Gamble," by Brenner, 1299
 Hitchcock, James (R), 422
 Hitler, Adolf, 152
 "Hitler und die Endlösung," by Fleming, 152
 "Hitler's Luftwaffe in the Spanish Civil War," by Proctor, 1352
 Ho Tai, Hue-Tam, "Millenarianism and Peasant Politics in Vietnam," 508
 Hoak, Dale (C), 588
 Hoare, Michael E., editor, "The Resolution Journal of Johann Reinhold Forster, 1772–1775," 120
 Hobsbawm, Eric, and Terence Ranger, editors, "The Invention of Tradition" (E), 241
 Hobson, Fred, "Tell about the South: The Southern Rage To Explain," 1381
 Hodges, Richard, and David Whitehouse, "Mohammed, Charlemagne, and the Origins of Europe: Archaeology and the Pirenne Thesis," 1061
 Hoerder, Dirk, editor, "American Labor and Immigrant History, 1877–1920s: Recent European Research," 529
 Hoffer, Peter Charles, "Revolution and Regeneration: Life Cycle and the Historical Vision of the Generation of 1776," 1389
 Hoffman, Abraham (R), 838
 Hoffman, Hildegard, *et al.*, editors, "Jahrbuch für Geschichte des Feudalismus" (E), 1198
 Hoffman, Ronald, and Ira Berlin, editors, "Slavery and Freedom in the Age of the American Revolution," 512
 Hoffman, Ronald, and Peter J. Albert, editors, "Arms and Independence: The Military Character of the American Revolution," 1387
 Hoffmann, Tamás, and Péter Gunst, "Large Estates and Small Holdings in Europe in the Middle Ages and Modern Times: National Reports" (E), 243
 Hofstadter, Richard, 976
 Hogan, Michael J. (R), 1407
 Hohenberg, Paul M. (R), 782
 Hohlfelder, Robert L., editor, "City, Town, and Countryside in the Early Byzantine Era," 744
 Hoisington, William A., Jr. (R), 784
 Hoisington, William A., Jr., "The Casablanca Connection: French Colonial Policy, 1936–1943," 1339
 Holbo, Paul S., "Tarnished Expansion: The Alaska Scandal, the Press, and Congress, 1867–1871," 526
 Hollander, Paul (C), 1223
 Holley, Donald (R), 521
 Holley, I. B., Jr., "General John M. Palmer, Citizen Soldiers, and the Army of a Democracy," 225
 Hollis, Daniel W. (R), 210
 Hollis, Daniel Webster, III, "An Alabama Newspaper Tradition: Grover C. Hall and the Hall Family," 876
 Hollos, Marida, and Bela C. Maday, editors, "New Hungarian Peasants: An East Central European Experience with Collectivization" (E), 1200
 Holmes, Colin (R), 1079
 Holmes, Geoffrey, "Augustan England: Professions, State, and Society, 1680–1730," 119
 Holmes, Oliver W. (R), 141
 Holt, Michael F. (R), 1158
 Holum, Kenneth G., "Theodosian Empresses: Women and Imperial Dominion in Late Antiquity," 415
 "The Holy Greyhound," by Schmitt, 1063
 "Holy Land Pilgrimage in the Later Roman Empire, A.D. 312–460," by Hunt, 106
 "Holy Violence," by Perinbam, 1129
 Holzer, Harold, *et al.*, "The Lincoln Image: Abraham Lincoln and the Popular Print," 1395
 Homan, Gerlof D. (R), 1341
 Home, Robert, "City of Blood Revisited: A New Look at the Benin Expedition of 1897," 180
 "Homosexuality in Renaissance England," by Bray, 421
 Homze, Edward L. (R), 155
 Honeyman, Katrina, "Origins of Enterprise: Business Leadership in the Industrial Revolution," 435
 Hood, John Bell, 844
 "Hoover and the Un-Americans," by O'Reilly, 873
 Hoover, Franklin Delano, 67; 545
 Hoover, J. Edgar, 873
 Hopkins, Donald R., "Princes and Peasants: Smallpox in History," 1049
 Hopkins, Keith, "Death and Renewal," 741
 Horak, Stephan M. (R), 1115
 Horn, James J. (R), 565
 Horsman, Reginald (R), 1153
 Horwitch, Mel, "Clipped Wings: The American SST Conflict," 553
 "The House of Lords, 1603–1649," by Foster, 1067
 Hovannisian, Richard G. (R), 1401
 "How New York Stole the Idea of Modern Art," by Guilbaut, 1177

- "How the North Won," by Hattaway and Jones, 207
 "How the PhD Came to Britain," by Simpson, 1079
 Howard, George William Frederick (Viscount Morpeth), 432
 Howard, Victor B., "Black Liberation in Kentucky: Emancipation and Freedom, 1862–1884," 1396
 Howe, Irving, editor, "Alternatives: Proposals for America from the Democratic Left" (E), 1431
 Howe, Joseph, 235
 Howell, Cicely, "Land, Family, and Inheritance in Transition: Kibworth Harcourt, 1280–1700," 1070
 Howell, P. A., "The Judicial Committee of the Privy Council, 1833–1876: Its Origins, Structure, and Development," 437
 Howell, Roger, Jr. (R), 1066
 Hromník, Cyril A., "Indo-Africa: Towards a New Understanding of the History of Sub-Saharan Africa," 822
 Huber, Thomas M. (R), 827
 Hughes, H. Stuart, "Prisoners of Hope: The Silver Age of the Italian Jews, 1924–1974," 802
 Hughes, Judith M., "Emotion and High Politics: Personal Relations at the Summit in Late Nineteenth-Century Britain and Germany," 407
 Hughes, Thomas P., "Networks of Power: Electrification in Western Society, 1880–1930," 406
 Hull, Richard W. (R), 825
 "Humanists and Holy Writ," by Bentley, 1039
 Humes, Walter M., and Hamish M. Paterson, editors, "Scottish Culture and Scottish Education, 1800–1980" (E), 246
 "A Hundred Years Ago," by Ford and Harrison, 434
 "Hungary and the Jews," by Katzburg, 166
 Hunt, E. D., "Holy Land Pilgrimage in the Later Roman Empire, A.D. 312–460," 106
 Hunt, Michael H., "The Making of a Special Relationship: The United States and China to 1914," 232
 Hunt, William, "The Puritan Moment: The Coming of Revolution in an English County," 117
 Hunter, Michael, "Science and Society in Restoration England," 763
 Hunter, Robert, 1387
 "Hunter-Killer," by Y'Blood, 1406
 Hurst, Willard (R), 1398
 Hurwitz, Ellen Stiskin (R), 487
 Hurwitz, Leon, editor, "The Harmonization of European Public Policy: Regional Responses to Transnational Challenges" (E), 896
 Husung, Hans-Gerhard, "Protest und Repression im Vormärz: Norddeutschland zwischen Restauration und Revolution," 1100
 Hutchings, Robert L., "Soviet–East European Relations: Consolidation and Conflict, 1968–1980," 1362
 Hutchinson, Thomas, 194
 Huttenback, Robert A. (R), 831
 Hyde, Edward (Earl of Clarendon), 1326
 "The Hypocrisy of Justice in the Belle Époque," by Martin, 1337
 "The Idea of Poverty," by Himmelfarb, 1073
 "L'Idée de royauté à Rome," by Martin, 413
 "Ideologie und Politik der Muslimbrüder Syriens," by Reissner, 819
 "Ideology and the Rise of Labor Theory in America," by Debrizzi, 863
 Iggers, Georg G. (R), 468
 Ike, Nobutaka (R), 828
 Il'ina, G. I., "Kul'turnoe stroitel'stvo v Petrograde, Oktiabr' 1917–1920 gg.," 493
 Illeritskaia, E. V., "Agrarnyi vopros: Proval agrarnykh programm i politiki neproletarskikh partii v Rossii," 171
 Imhof, Arthur E., "Die gewonnenen Jahre: Von der Zunahme unserer Lebensspanne seit dreihundert Jahren *oder* von der Notwendigkeit einer neuen Einstellung zu Leben und Sterben," 409
 "An Immigrant Bishop," by Carey, 851
 "Immigrant Workers in Industrial France," by Cross, 784
 "Immigrants and the Class Struggle," by Buckman, 1079
 IMMIGRATION AND EMIGRATION: Abella and Troper, "None is Too Many," 886; Bade, "Vom Auswanderungsland zum Einwanderungsland?" 795; Buckman, "Immigrants and the Class Struggle," 1079; Engman, "St. Petersburg och Finland," 1097; Genizi, "American Apathy," 1406; Keil and Jentz, editors, "German Workers in Industrial Chicago, 1850–1910," 1164; Murphy, "Guestworkers in the German Reich," 1351; Poussou, "Bordeaux et le sud-ouest au XVIII^e siècle," 1083; Puskás, "From Hungary to the United States, 1880–1914," 165; Richardson, "Caribbean Migrants," 562; Sonne, "Saints on the Seas," 851
 "The Imperfect Peasant Economy," by Dallas, 135
 "The Imperial Imagination," by Wurgaft, 831
 "The Imperial Osages," by Din and Nasatir, 1149
 IMPERIALISM: Black, "A Gambling Style of Government," 509; Halstead, "The Second British Empire," 1076; Kamerkar, "British Paramountcy," 1138; Löhken, "Ordines Dignitatum," 742; Moore, "Escape from Empire," 439; Page, "Prelude to Partition," 505
 "The Implications of Literacy," by Stock, 107
 "L'Importance de l'exploration maritime au siècle des lumières," by Mollat *et al.* (E), 246
 "The Impossible Dream," by Johnpoll, 191
 "Impressions of a Gilded Age," edited by Chénétier and Kroes (E), 900
 "In Search of History," by Van Seters, 412
 "In the Shadow of FDR," by Leuchtenburg, 1185
 "The Inca and Aztec States, 1400–1800," edited by Collier *et al.*, 559
 "Indentured Labour in the British Empire, 1834–1920," edited by Saunders (E), 1426
 "Independence and Empire," by Hearden, 211
 "Independence and Exegesis," by Jones, 1292
 "The Indian Arts and Crafts Board," by Schrader, 871
 "Indian Merchants and the Decline of Surat, c. 1700–1750," by Das Gupta, 829
 "Indo-Africa," by Hromník, 822
 INDUSTRIALIZATION: Davis, "Challenging Colonialism," 498; Feldenkirchen, "Die Eisen- und Stahlindustrie des Ruhrgebiets, 1879–1914," 147; Gregory, "Regional Transformation and Industrial Revolution," 1074; Hamerow, "The Birth of a New Europe," 736; Jenkins and Ponting, "The British Wool Textile Industry, 1770–1914," 1074; Nolte, "Marxismus und Industrielle Revolution," 1041; Prude, "The Coming of Industrial Order," 1157; Ray, "Industrialization in India," 507; Veyrassat, "Négociants et fabricants dans l'industrie cotonnière suisse, 1760–1840," 797; Ward and Ward, *Infant Birth Weight and Nutrition in Industrializing Montreal*, 324–45
 "Industrialization in India," by Ray, 507

- Infant Birth Weight and Nutrition in Industrializing Montreal*, by Ward and Ward, 324–45
- "Inflation through the Ages," edited by Schmukler and Marcus (E), 894
- "The Influence of the Carnegie, Ford, and Rockefeller Foundations on American Foreign Policy," by Berman, 1184
- Inkster, Ian, and Jack Morrell, editors, "Metropolis and Province: Science in British Culture, 1780–1850" (E), 245
- "Innenpolitische Probleme des Bismarck-Reiches," edited by Pflanze (E), 247
- Innes, Stephen, "Labor in a New Land: Economy and Society in Seventeenth-Century Springfield," 840
- Inoue, Tadakatsu, and Akio Okochi, editors, "Overseas Business Activities" (E), 1426
- INSTITUTIONAL HISTORY: Booraem, "The Formation of the Republican Party in New York," 1391; Brenzel, "Daughters of the State," 1169; Church, "Revolution and Red Tape," 452; Conner, "The National War Labor Board," 865; Diefendorf, "Paris City Councillors in the Sixteenth Century," 131; England, "A Patron for Pure Science," 102; Foster, "The House of Lords, 1603–1649," 1067; Freedman, "The Diocese of Vic," 1319; Gallet, "La seigneurie bretonne, 1450–1680," 1332; Graf, "Politische Polizei zwischen Demokratie und Diktatur," 467; Hernández Esteve, "Creación del consejo de hacienda de Castilla, 1523–1525," 1341; Keve, "The McNeil Century," 1166; Koroleva, "Pervaia rossiiskaia revoliutsiia i tsarizm," 490; Lagemann, "Private Power for the Public Good," 551; Lichtenstein, "Labor's War at Home," 866; Linz, "Der Bund der Landwirte in der Ersten Tschechoslowakischen Republik," 167; MacLeod, "Building Character in the American Boy," 1169; Maiorini, "Il Movimento Repubblicain Populaire partito della IV Repubblica," 1086; Meiners, "A History of Rice University," 881; Mosher, "A Tale of Two Agencies," 1412; Neuhaus, "Reichsständische Repräsentationsformen im 16. Jahrhundert," 463; O'Reilly, "Hoover and the Un-Americans," 873; Pichardo, "Biografía del Colegio de San Cristóbal de La Habana," 1422; Pocock, *Presidents of the American Historical Association: A Statistical Analysis* (Research Note), 1016–24; Raichle, "New Jersey's Union College," 1408; Rooke and Schnell, "Discarding the Asylum," 1415; Rothberg, "To Set a Standard of Workmanship and Compel Men to Conform to it": John Franklin Jameson as Editor of the American Historical Review, 957–75; Sass, "The Pragmatic Imagination," 216; Scanlon, "Randolph-Macon College," 881; Sedlak and Williamson, "The Evolution of Management Education," 1409; Shafer, "Quiet Revolution," 1414; Silber, "Test and Protest," 874; Skardon, "Steel Valley University," 1408; Van Tassel, *From Learned Society to Professional Organization: The American Historical Association, 1884–1900*, 929–56; White, "The Regulation and Reform of the American Banking System, 1900–1929," 217
- "Institutionen, Kultur und Gesellschaft im Mittelalter," edited by Fenske et al. (E), 1427
- "Insurrection," by Wells, 766
- "The Intellectual Crisis in English Catholicism," by Schoenl, 441
- INTELLECTUAL HISTORY: Beyer, "Nature et valeur dans la philosophie de Montesquieu," 451; Blumenthal, editor, "Carolingian Essays," 752; Bogin, "Abraham Clark and the Quest for Equality in the Revolutionary Era, 1774–1794," 194; Brantlinger, "Bread and Circuses," 1305; Brooks, "Just before the Origin," 1297; Bryce, "Cosimo Bartoli, 1503–1572," 1108; Cérullo, "The Secularization of the Soul," 444; Colaïaco, "James Fitzjames Stephen and the Crisis of Victorian Thought," 122; Cuevas Cancino, "Bolívar en el tiempo," 1416; Dowler, "Dostoevsky, Grigor'ev, and Native Soil Conservatism," 169; Eliav-Feldon, "Realistic Utopias," 91; Hampson, "Will and Circumstance," 1084; Hansen, "Disaffection and Decadence," 137; Hellman, "Simone Weil," 446; Heyd, "Between Orthodoxy and the Enlightenment," 474; Hirsh, "The French New Left," 141; Keeding, "Das Zeitalter der Aufklärung in der Provinz Quito," 1419; King, "The Iron of Melancholy," 1151; Lerner, "The Powers of Prophecy," 746; Loetscher, "Facing the Enlightenment and Pietism," 202; Logan, "The Meaning of More's *Utopia*," 91; Lowe, "History of Bourgeois Perception," 1301; Major-Poetzl, "Michel Foucault's Archaeology of Western Culture," 90; Marrone, "William of Auvergne and Robert Grosseteste," 748; Moyer, "American Physics in Transition," 1163; Ninkovich, "The Diplomacy of Ideas," 883; Paulson, "Language, Science, and Action," 1308; Pencak, "America's Burke," 194; Perinbam, "Holy Violence," 1129; Pusey, "China and Charles Darwin," 1135; Ross, *Historical Consciousness in Nineteenth-Century America*, 909–28; Rubanowice, "Crisis in Consciousness," 791; Schmitt, "John Case and Aristotelianism in Renaissance England," 757; Shapiro, "Probability and Certainty in Seventeenth-Century England," 762; Simms, "William Molyneux of Dublin, 1656–1698," 129; Smith, "Barrington Moore, Jr.," 403; Thompson, "Winston Churchill's World View," 125; Walsh, "Growing Up in British India," 831; Wilson, "The Benevolent Deity," 1385; Wootton, "Paolo Sarpi," 1109
- "International Labour Migration," edited by Marks and Richardson (E), 1425
- INTERNATIONAL RELATIONS: Barratt, "Russian Shadows on the British Northwest Coast of North America, 1810–1890," 1044; Chan, "Arming the Chinese," 183; Cohen, "Palestine and the Great Powers, 1945–1948," 177; Cohen, editor, "New Frontiers in American–East Asian Relations," 884; Cole, "Gunboats and Marines," 538; Egan, "Neither Peace Nor War," 198; Ekirch, *Great Britain's Secret Convict Trade to America, 1783–1784* (Research Note), 1285–91; Grabbe, "Unionsparteien, Sozialdemokratie und Vereinigte Staaten von Amerika, 1945–1966," 155; Grayson, "United States–Iranian Relations," 885; Hunt, "The Making of a Special Relationship," 232; Hutchings, "Soviet–East European Relations," 1362; Leffler, *The American Conception of National Security and the Beginnings of the Cold War, 1945–48* (Forum), 346–81; Marcus, "Ethiopia, Great Britain, and the United States, 1941–1974," 1132; Palmer, "The Chancelleries of Europe," 1045; Parks, "Culture, Conflict, and Coexistence," 1123; Reed, "The Missionary Mind and American East Asia Policy, 1911–1915," 537; Rösch, "Venedig und das Reich," 115; Seymour, "Anglo-Danish Relations and Germany, 1933–1945," 461; Ulam, "Dangerous Relations," 174; Wexler, "The Marshall Plan Revisited," 1407
- "Interpreting Indonesian Politics," edited by Anderson and Kahin (E), 251
- "Introducción general a la historia de la Iglesia en América latina," vol. 1, pt. 1 of "Historia general de la Iglesia en América latina," by Dussel, 1416

- "The Invention of Tradition," edited by Hobsbawm and Ranger (E), 241
 "The Invisible Farmers," by Sachs, 548
 "Irish Peasants," edited by Clark and Donnelly (E), 897
 Iriye, Akira (R), 1180
 "The Iron of Melancholy," by King, 1151
 Irons, Peter, "Justice at War," 871
 Irvine, William D. (R), 1086
 Isaac, Rhys (R), 1145
 Isaacs, Rufus (Lord Reading), 438
 "Isfahan im 15. und 16. Jahrhundert," by Quiring-Zoche, 499
 Ishaq Khan, Mohammad, "Perspectives on Kashmir: Historical Dimensions," 1378
 ISLAM: Chejne, "Islam and the West," 1064; Lambton, "State and Government in Medieval Islam," 1125; Lindner, "Nomads and Ottomans in Medieval Anatolia," 1124; Müller, "Die Kunst des Sklavenkaufs," 818; Musallam, "Sex and Society in Islam," 1363; Reissner, "Ideologie und Politik der Muslimbrüder Syriens," 819; Smith, "Islam and the Search for Social Order in Modern Egypt," 1128; Van Dijk, "Rebellion under the Banner of Islam," 190
 "Islam and the Search for Social Order in Modern Egypt," by Smith, 1128
 "Islam and the West," by Chejne, 1064
 "The Islamic Impact," edited by Haddad *et al.* (E), 1430
 "Islamic Revivalism in a Changing Peasant Economy," by Dobbin, 189
 Ismael, Jacqueline S. (R), 819
 "Israel in the Mind of America," by Grose, 1384
 Issawi, Charles, "The Economic History of Turkey, 1800–1914," 497
 "Istoria parlamentului si a vietii parlamentare din România pînă la 1918," edited by Căncea *et al.*, 1110
 "Istoria tou Ellinotourkikou Horou apo ton E. Venizelo ston g. Papadopoulos, 1928–1973," by Kitsikis, 803
 "Istoriia vtoroi mirovoi voiny, 1939–1945," vol. 12, "Itogi i uroki vtoroi mirovoi voiny," by Tiushkevich *et al.*, 494
 "Italy and the Approach of the First World War," by Bosworth, 801
 Itkonen, Esa, *et al.*, editors, "Ajatuksen ja toiminnan tiet: Matti Juntusen muistikirja" (E), 250
 "Itogi i uroki vtoroi mirovoi voiny," vol. 12 of "Istoriia vtoroi mirovoi voiny, 1939–1945," by Tiushkevich *et al.*, 494
 Itzkowitz, Norman (R), 1124
 Iverson, Peter (R), 858
 Ives, E. W., "The Common Lawyers of Pre-Reformation England: Thomas Kebell, a Case Study," 756
 Ivie, Robert L., and Ronald L. Hatzenbuehler, "Congress Declares War: Rhetoric, Leadership, and Partisanship in the Early Republic," 1391
 J. Franklin Jameson, Carter G. Woodson, and the *Foundations of Black Historiography* (Research Note), by Meier and Rudwick, 1005–15
 Jablon, Howard, "Crossroads of Decision: The State Department and Foreign Policy, 1933–1937," 1404
 Jackman, S. W. (R), 235
 Jackson, George (R), 815
 Jacob, James R., "Henry Stubbe, Radical Protestantism, and the Early Enlightenment," 426
 Jacob, Margaret C. (R), 762
 Jacobson, Jon (C), 586
 "Jahrbuch für Geschichte des Feudalismus," edited by Hoffman *et al.* (E), 1198
 "Die Jahre weiss man nicht, wo man die heute hinsetzen sol," edited by Niethammer, 795
 "James Fitzjames Stephen and the Crisis of Victorian Thought," by Colaiaico, 122
 Jameson, J. Franklin, 957; 1005
 Jancey, Meryl, editor, "St. Thomas Cantilupe, Bishop of Hereford: Essays in His Honour" (E), 245
 Jansen, Marius B. (R), 186
 "Japan and the San Francisco Peace Settlement," by Yoshitsu, 1377
 "Japan's Local Pragmatists," by Waters, 827
 "Japanese Workers and the Struggle for Power, 1945–1947," by Moore, 828
 Jarausch, Konrad H., editor, "The Transformation of Higher Learning, 1860–1930: Expansion, Diversification, Social Opening, and Professionalization in England, Germany, Russia, and the United States" (E), 570
 "Jedidiah Morse and New England Congregationalism," by Phillips, 515
 Jeffrey, Robin (R), 439
 Jeffreys-Jones, Rhodri, editor, "Eagle against Empire: American Opposition to European Imperialism, 1914–1982" (E), 1431
 Jeismann, Karl-Ernst, and Rainer Riemenschneider, editors, "Polen und Deutschland im Zeitalter der Aufklärung: Reformen im Bereich des politischen Lebens, der Verfassung und der Bildung" (E), 247
 Jelavich, Barbara (R), 1045
 Jelavich, Barbara, "History of the Balkans," vol. 1, "Eighteenth and Nineteenth Centuries"; vol. 2, "Twentieth Century," 1116
 Jelinek, Lawrence J. (R), 856
 Jelinek, Yeshayahu A., "The Lust for Power: Nationalism, Slovakia, and the Communists, 1918–1948," 1113
 Jenkins, D. T., and K. G. Ponting, "The British Wool Textile Industry, 1770–1914," 1074
 Jenkins, Philip, "The Making of a Ruling Class: The Glamorgan Gentry, 1640–1790," 765
 Jennings, James, and Monte Rivera, editors, "Puerto Rican Politics in Urban America" (E), 1431
 Jensen, Peter Hoxcer, *et al.*, editors, "Dansk kolonihistorie: Indføring og studier" (E), 1429
 Jentz, John B., and Hartmut Keil, editors, "German Workers in Industrial Chicago, 1850–1910: A Comparative Perspective," 1164
 "Jesuit Ranches and the Agrarian Development of Colonial Argentina, 1650–1767," by Cushner, 1419
 "The Jewish Community in British Politics," by Alderman, 440
 "Jewish Origins of the Psychoanalytic Movement," by Klein, 408
 "Jewish Thought in the Sixteenth Century," edited by Cooperman (E), 1195
 "The Jews of East Central Europe between the World Wars," by Mendelsohn, 737
 "The Jews of Europe and the Inquisition of Venice, 1550–1670," by Pullan, 1109
 "Joao de Barros," by Boxer, 459
 "Job Studies and Industrial Relations," by De Geer, 158
 Johannsen, Robert W. (R), 1158
 "John Bell Hood and the War for Southern Independence," by McMurtry, 844
 "John Case and Aristotelianism in Renaissance England," by Schmitt, 757
 "John Chrysostom and the Jews," by Wilken, 1059

- "John Foxe," by Wooden, 758
 "John Muir and His Legacy," by Fox, 223
 Johnpoll, Bernard K., "The Impossible Dream: The Rise and Demise of the American Left," 191
 Johns, Elizabeth, "Thomas Eakins: The Heroism of Modern Life," 1398
 Johnson, Daniel M., and Rex R. Campbell, "Black Migration in America: A Social Demographic History," 542
 Johnson, Michael P. (R), 1392
 Johnson, Paul Barton (R), 125
 Johnson, Penelope D. (R), 1602
 Johnson, R. E. (R), 490; 1117
 Johnson, Robert Erwin (R), 849
 Johnson, Robert Erwin, "Far China Station: The U.S. Navy in Asian Waters, 1800–1898," 517
 Johnson, Stephen, "Late Roman Fortifications," 743
 Johnson, William A. (R), 507
 Jokipii, Mauno, and Ilkka Nummela, editors, "Ur Nordisk kulturhistoria: Den kritiska tanken i vetenskapen på 1700- och 1800-talen" (E), 249
 Jonas, Manfred (R), 538
 "Jonathan Daniels and Race Relations," by Eagles, 224
 Jones, Alan H., "Independence and Exegesis: The Study of Early Christianity in the Work of Alfred Loisy (1857–1940), Charles Guignebert (1857–1939), and Maurice Goguel (1880–1955)," 1292
 Jones, Archer, and Herman Hattaway, "How the North Won: A Military History of the Civil War," 207
 Jones, Colin, "Charity and 'Bienfaisance': The Treatment of the Poor in the Montpellier Region, 1740–1815," 132
 Jones, Colin, editor, "Britain and Revolutionary France: Conflict, Subversion, and Propaganda" (E), 245
 Jones, E. L. (R), 755
 Jones, Michael (R), 113
 Jones, Norman L., "Faith by Statute: Parliament and the Settlement of Religion, 1559," 1325
 Jones, Robert Leslie, "History of Agriculture in Ohio to 1880," 1161
 Jones, Tom B. (R), 739
 Jordan, Daniel P. (R), 514
 Jordan, Daniel P., "Political Leadership in Jefferson's Virginia," 1152
 Jordan, Philip D., "The Evangelical Alliance for the United States of America, 1847–1900: Ecumenism, Identity, and the Religion of the Republic," 861
 Jordan, William Chester (R), 751
 Joselit, Jenna Weissman, "Our Gang: Jewish Crime and the New York Jewish Community, 1900–1940," 1179
 "Joseph Howe," vol. 1, "Conservative Reformer, 1804–1848," by Beck, 235
 Joseph II, 250
 JOURNALISM AND THE MEDIA: Feyel, "La 'Gazette' en province à travers ses réimpressions, 1631–1752," 779; Holbo, "Tarnished Expansion," 526; Hollis, "An Alabama Newspaper Tradition," 876; Mitchell, "Censorship in Imperial Japan," 827; Rutherford, "A Victorian Authority," 237; Salokangas, "Puolueen aseet," 790; Sanders and Taylor, "British Propaganda during the First World War, 1914–18," 1081; Sibley, "Lone Stars and State Gazettes," 524; Sperlich, "Journalist mit Mandat," 1350
 "Journalist mit Mandat," by Sperlich, 1350
 "Journey from Jim Crow," by Barnes, 1411
 "Journey to North Korea," edited by Kim and Koh (E), 577
 Jouve, Michel, "L'Âge d'or de la caricature anglaise," 770
 Joyce, Donald Franklin, "Gatekeepers of Black Culture: Black-Owned Book Publishing in the United States, 1817–1981," 211
 Joyce, William L., *et al.*, editors, "Printing and Society in Early America" (E), 899
 "JPL and the American Space Program," by Koppes, 554
 JUDAISM: Alderman, "The Jewish Community in British Politics," 440; Buckman, "Immigrants and the Class Struggle," 1079; Davis, "The English Rothschilds," 1077; Eisen, "The Chosen People in America," 1178; Fleming, "Hitler und die Endlösung," 152; Goitein, "A Mediterranean Society," vol. 4, "Daily Life," 1365; Gorny, "The British Labour Movement and Zionism, 1917–1948," 441; Grose, "Israel in the Mind of America," 1384; Hughes, "Prisoners of Hope," 802; Joselit, "Our Gang," 1179; Katzburg, "Hungary and the Jews," 166; Klein, "Jewish Origins of the Psychoanalytic Movement," 408; Kornberg, editor, "At the Crossroads," 1366; Laskier, "The Alliance Israélite Universelle and the Jewish Communities of Morocco," 1371; Mendelsohn, "The Jews of East Central Europe between the World Wars," 737; Pollins, "Economic History of the Jews in England," 1078; Pullan, "The Jews of Europe and the Inquisition of Venice, 1550–1670," 1109; Septimus, "Hispano-Jewish Culture in Transition," 420; Stanislawski, "Tsar Nicholas I and the Jews," 810; Urofsky, "A Voice That Spoke for Justice," 225
 Judd, Denis, "Lord Reading: Rufus Isaacs, First Marquess of Reading, Lord Chief Justice, and Viceroy of India, 1860–1935," 438
 "Die Juden in den böhmischen Ländern," edited by Seibt (E), 1429
 Judge, Edward H., "Plehve: Repression and Reform in Imperial Russia, 1902–1904," 812
 "The Judicial Committee of the Privy Council, 1833–1876," by Howell, 437
 "Jugoslovenska misao i socijalizam," by Redžić, 804
 "Julius Caesar and His Public Image," by Yavetz, 741
 Juntunen, Matti, 250
 "Just before the Origin," by Brooks, 1297
 "Justice at War," by Irons, 871
 "Justice by Insurance," by Borah, 1418
 "Justice without Law?" by Auerbach, 533
 Kabisch, Thomas R., "Deutsches Kapital in den USA: Von der Reichsgründung bis zur Sequestrierung (1917) und Freigabe," 233
 Kaegi, Walter Emil, Jr. (R), 1060
 Kaelble, Hartmut, "Soziale Mobilität und Chancengleichheit im 19. und 20. Jahrhundert: Deutschland im internationalen Vergleich," 1302
 Kagan, Donald (R), 1311
 Kahin, Audrey, and Benedict Anderson, editors, "Interpreting Indonesian Politics: Thirteen Contributions to the Debate" (E), 251
 Kaiser, Daniel H. (R), 808
 Kamerkar, Mani, "British Paramouncy: British-Baroda Relations, 1818–1848," 1138
 Kaminsky, Catherine, and Simon Kruk, "Le nationalisme arabe et le nationalisme juif," 1364
 Kaminsky, Howard, "Simon de Cramaud and the Great Schism," 753
 Kamoche, Jidolph G. (R), 824

- "Der Kampf der Tschechen um die historischen Rechte der böhmischen Krone im Spiegel der Presse, 1861–1879," by Von Falkenstein, 166
- Kantorowicz, Ernst, 466
- Kantowicz, Edward R., "Corporation Sole: Cardinal Mundelein and Chicago Catholicism," 221
- "Kapital, Reichswehr und NSDAP, 1919–1924," by Gossweiler, 148
- Kaplan, Lawrence (R), 425
- Kaplan, Lawrence S. (R), 234
- Karamanski, Theodore J., "Fur Trade and Exploration: Opening the Far Northwest, 1821–1852," 556
- Karant-Nunn, Susan C. (R), 463; 1346
- Kasson, John F. (R), 1172
- Kater, Michael H. (R), 1102
- Kater, Michael H., "The Nazi Party: A Social Profile of Members and Leaders, 1919–1945," 469
- "Katholischer Gesellenverein und deutsche Kolpingfamilie in der Ära des Nationalsozialismus," by Raem, 145
- Kätz, Barry M. (R), 403
- Katz, Stanley N. (R), 1149
- Katzburg, Nathaniel, "Hungary and the Jews: Policy and Legislation, 1920–1943," 166
- Katzenstein, Mary Fainsod, "Ethnicity and Equality: The Shiv Sena Party and Preferential Policies in Bombay," 508
- Kaufman, Allen, "Capitalism, Slavery, and Republican Values: Antebellum Political Economists, 1819–1848," 520
- Kaufman, Burton (R), 884
- Kaufman, Burton I., "Trade and Aid: Eisenhower's Foreign Economic Policy, 1953–1961," 550
- Kautsky, John H., "The Politics of Aristocratic Empires," 1043
- Kea, Ray A., "Settlements, Trade, and Politics in the Seventeenth-Century Gold Coast," 823
- Kearney, Hugh (R), 119
- Kebell, Thomas, 756
- Kedar, B. Z., *et al.*, editors, "Outremer: Studies in the History of the Crusading Kingdom of Jerusalem: Presented to Joshua Prawer," 111
- Keddie, Nikki (C), 585
- Kedourie, Elie, and Sylvia G. Haim, editors, "Zionism and Arabism in Palestine and Israel," 497
- Kee, Howard Clark, "Miracle in the Early Christian World: A Study in Sociohistorical Method," 1292
- Keeding, Ekkehart, "Das Zeitalter der Aufklärung in der Provinz Quito," 1419
- Keefe, Thomas K., "Feudal Assessments and the Political Community under Henry II and His Sons," 1320
- Keen, Benjamin (R), 1188
- Keightley, David N., editor, "The Origins of Chinese Civilization" (E), 252
- Keil, Hartmut, and John B. Jentz, editors, "German Workers in Industrial Chicago, 1850–1910: A Comparative Perspective," 1164
- "Kein einzig Volk von Brüdern," by Arbeitsgemeinschaft Volksgruppenfrage (E), 247
- Kellenbenz, Hermann (R), 465
- Keller, Robert H., Jr., "American Protestantism and United States Indian Policy, 1869–1882," 208
- Kellner, Hans (R), 1048; 1308
- Kellogg, Frederick (R), 1110
- Kelly, Lawrence C., "The Assault on Assimilation: John Collier and the Origins of Indian Policy Reform," 870
- Kemmerer, Donald L., 242
- Kemp, Tom (R), 135
- Kempe, Margery, 749
- Kempf, Friedrich, 1427
- Kendle, John (R), 1076
- Kennedy, Paul (R), 407; 1362
- Kennedy, R. Emmet, Jr. (R), 1084
- Kent, D. V., and F. W. Kent, "Neighbours and Neighbourhood in Renaissance Florence: The District of the Red Lion in the Fifteenth Century," 1107
- Kent, F. W., and D. V. Kent, "Neighbours and Neighbourhood in Renaissance Florence: The District of the Red Lion in the Fifteenth Century," 1107
- Kent, George O. (R), 150
- "Kentucky Presbyterians," by Weeks, 1384
- Keppie, Lawrence, "Colonisation and Veteran Settlement in Italy, 47–14 B.C.," 413
- Kermode, J. I., and C. B. Phillips, editors, "Seventeenth-Century Lancashire: Essays presented to J. J. Bagley," 1326
- Kern, Horst, "Empirische Sozialforschung: Ursprünge, Ansätze, Entwicklungslinien," 142
- Kern, Stephen, "The Culture of Time and Space, 1880–1918," 736
- Kerr, Donal A., "Peel, Priests, and Politics: Sir Robert Peel's Administration and the Roman Catholic Church in Ireland, 1841–1846," 449
- Kerr, Howard, and Charles L. Crow, editors, "The Occult in America: New Historical Perspectives" (E), 899
- Kershaw, Ian, "Popular Opinion and Political Dissent in the Third Reich: Bavaria, 1933–1945," 793
- Kessler, Clive S. (R), 1379
- Kessler-Harris, Alice, "Out to Work: A History of Wage-Earning Women in the United States," 193
- Kettenacker, Lothar, and Wolfgang J. Mommsen, editors, "The Fascist Challenge and the Policy of Appeasement" (E), 568
- Keve, Paul W., "The McNeil Century: The Life and Times of an Island Prison," 1166
- Key, Kerim K. (R), 176
- "Khans and Shahs," by Garthwaite, 1368
- Khuri, Fuad I., "Tribe and State in Bahrain: The Transformation of Social and Political Authority in an Arab State," 1368
- Kicza, John E., "Colonial Entrepreneurs: Families and Business in Bourbon Mexico City," 1189
- "Kievskaja Rus' i russkie kniazhestva, XII–XIII vv.," by Rybakov, 487
- Kiger, Joseph C. (R), 222; 551
- "Kikkoman," by Fruin, 1137
- Kim, C. I. Eugene, and B. C. Koh, editors, "Journey to North Korea: Personal Perceptions" (E), 577
- Kim, Jon-Yop, and Robert A. Scalapino, editors, "North Korea Today: Strategic and Domestic Issues" (E), 577
- Kimball, H. I., 526
- Kimmich, Christoph M. (R), 473
- Kindleberger, Charles P. (R), 437; 1302
- "King Charles I," by Gregg, 760
- "King Edward III," by Packe, 749
- King, John Owen, III, "The Iron of Melancholy: Structures of Spiritual Conversion in America from the Puritan Conscience to Victorian Neurosis," 1151
- King, Preston, editor, "The History of Ideas: An Introduction to Method" (E), 1425
- King, W. James (R), 406
- "The Kingdom of Kongo," by Thornton, 1130
- "Kings and Vikings," by Sawyer, 745
- "Kings, Commoners, and Concessionaires," by Bonner, 825

- Kingston-Mann, Esther, "Lenin and the Problem of Marxist Peasant Revolution," 1120
- Kinmonth, Earl H. (R), 1376
- Kinner, Klaus, "Marxistische deutsche Geschichtswissenschaft, 1917 bis 1933: Geschichte und Politik im Kampf der KPD," 468
- Király, Béla K., 569
- Kirschbaum, Joseph M., 899
- Kirschbaum, Stanislav (R), 1113
- Kirschbaum, Stanislav J., editor, "Slovak Politics: Essays on Slovak History in Honour of Joseph M. Kirschbaum" (E), 899
- Kitsikis, Dimitri (R), 802
- Kitsikis, Dimitri, "Istoria tou Ellinotourkikou Horou apo ton E. Venizelo ston g. Papadopoulos, 1928-1973," 803
- Kitsikis, Dimitri, "Synkritiki Istoria Ellados kai Tourkias stou 20% aiona," 803
- Klassen, John (R), 1102
- Klausner, Carla L. (R), 1061
- Kleber, Brooks E. (R), 225
- Klein, Dennis B., "Jewish Origins of the Psychoanalytic Movement," 408
- Klein, Herbert S. (C), 591
- Klein, Martin A., and Claire C. Robertson, editors, "Women and Slavery in Africa," 1370
- Kleppner, Paul, "Who Voted? The Dynamics of Electoral Turnout, 1870-1980," 214
- Klingenstein, Grete, and Heinrich Lutz, editors, "Spezialforschung und 'Gesamtgeschichte': Beispiele und Methodenfragen zur Geschichte der frühen Neuzeit" (E), 246
- "Die Klostersäkularisation in Kent, 1535-1558," by Schüler, 759
- Kluxen, Kurt, and Adolf M. Birke, editors, "Viktorianisches England in deutscher Perspektive" (E), 1198
- Knafl, Louis A., and French R. Fogle, "Patronage in Late Renaissance England" (E), 572
- Knight, George R., editor, "Early Adventist Educators" (E), 1202
- Knoll, Paul W. (R), 419
- Knorr, Klaus, editor, "Power, Strategy, and Security" (E), 242
- "Knoxville, Tennessee," by McDonald and Wheeler, 880
- Koblik, Steven (R), 1092
- Kofas, Jon V., "Authoritarianism in Greece: The Metaxas Regime," 802
- Koh, B. C., and C. I. Eugene Kim, editors, "Journey to North Korea: Personal Perceptions" (E), 577
- Koistinen, Paul A. C. (R), 233
- Kolb, Robert (R), 1040
- Kolkey, Jonathan Martin, "The New Right, 1960-1968, with Epilogue, 1969-1980," 1414
- Kollar, Rene M. (R), 1080
- Komlos, John, "The Habsburg Monarchy as a Customs Union: Economic Development in Austria-Hungary in the Nineteenth Century," 1103
- Komlos, John, editor, "Economic Development in the Habsburg Monarchy in the Nineteenth Century: Essays" (E), 250
- "Konfessionsbildung und Stände in Böhmen, 1478-1530," by Eberhard, 1102
- "Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog," vol. 10b, "Het laatste jaar II," by de Jong, 460
- Köpeczi, Béla, "Staatsräson und christliche Solidarität: Die ungarischen Aufstände und Europa in der zweiten Hälfte des 17. Jahrhunderts," 1111
- Kopf, David (R), 831
- Koppes, Clayton R., "JPL and the American Space Program: A History of the Jet Propulsion Laboratory," 554
- Kornberg, Jacques, editor, "At the Crossroads: Essays on Ahad Ha-Am," 1366
- Kornweibel, Theodore, Jr. (R), 211
- Koroleva, N. G., "Pervaia Rossiiskaia revoliutsiia i tsarizm: Sovet ministrov Rossii v 1905-1907 gg.," 490
- Korr, Charles P. (R), 1309
- Korshin, Paul J., "Typologies in England, 1650-1820," 116
- Korzybski, Count Alfred, 1308
- Kosáry, Domokos, "Széchenyi Döblingben," 164
- Koss, Stephen (R), 767
- Kossmann, E. H. (R), 403
- "Kouluylihallituksen ensimmäisen päällikön Casimir von Kothenin koulupolitiikka," by Härkönen, 1097
- Kousoulas, D. G. (R), 803
- Kousser, J. Morgan (R), 203; 854
- Kovacs, Martin L. (R), 165
- Kovacs, Martin L., editor, "Roots and Realities among Eastern and Central Europeans" (E), 250
- Koval'chenko, I. D., *et al.*, "Sotsial'no-ekonomicheskii stroi pomeschchich'ego khoziaistva Evropeiskoi Rossii v epokhu kapitalizma: Istochniki i metody izucheniia," 170
- Kraig, Bruce (R), 1310
- Kraszewski, Piotr, "Polityka wielkiej Brytanii wobec Niemiec w latach 1918-1925," 774
- Kraut, Alan M., editor, "Crusaders and Compromisers: Essays on the Relationship of the Antislavery Struggle to the Antebellum Party System," 1158
- "Krest'ianskaia obshchina v Rossii vo vtoroi polovine XVIII-pervoi polovine XIX v.," by Prokof'eva, 811
- "Krest'ianskoe khoziaistvo evropeiskoi Rossii, 1881-1904," by Anfimov, 811
- Kristensen, Anne K. G., "Tacitus' germanische Gefolgschaft," 743
- Kroes, Rob, and Marc Chénétier, editors, "Impressions of a Gilded Age: The American Fin de Siecle" (E), 900
- "Kronstadt, 1917-1921," by Getzler, 492
- "Kroz pet stoljeća Hrvatske povijesti," by Šidak, 1357
- Kruk, Simon, and Catherine Kaminsky, "Le nationalisme arabe et le nationalisme juif," 1364
- Kruman, Marc W., "Parties and Politics in North Carolina, 1836-1865," 849
- "Krushchev and the Communist World," edited by Miller and Féhér (E), 1429
- Kselman, Thomas A., "Miracles and Prophecies in Nineteenth-Century France," 782
- Kubicek, Robert (R), 1373
- Kubricht, A. Paul (R), 167
- Kuehn, Thomas (R), 418
- Kuklick, Henrika (R), 1130
- "Kultur der einfachen Leute," edited by Van Dülmen (E), 572
- "Kul'turnoe stroitel'stvo v Petrograde, Oktiabr' 1917-1920 gg.," by Il'ina, 493
- Kumar, Dharma, and Meghnad Desai, editors, "The Cambridge Economic History of India," vol. 2, "c. 1757-c. 1970," 829
- Kuniholm, Bruce, *Comment on Leffler, The American Conception of National Security and the Beginnings of the Cold War, 1945-48* (Forum), 385-90
- "Die Kunst des Sklavenkaufs," by Müller, 818
- Kurgan-van Hentenryk, G., "Rail, finance, et politique: Les entreprises Philippart, 1865-1890," 787
- Kürkcüoğlu Ömer, "Osmanlı Devleti'ne Karsi Arap Bagimsizlik Hareketi, 1908-1918," 176

- Kusin, Vladimir V. (C), 1219
 Kwak, Tae-Hwan, *et al.*, editors, "U.S.-Korean Relations, 1882-1982" (E), 1202
 Kyba, Patrick, "Covenants without the Sword: Public Opinion and British Defence Policy, 1931-1935," 775
- La Salle: Discovery of a Lost Explorer*, by Wood, 294-323
 La Salle, Sieur de (René-Robert Cavelier), 294
 "Het laatste jaar II," vol. 10b of "Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog," by de Jong, 460
 "Labor and Capital on the African Copperbelt," by Parpart, 1373
- LABOR AND LABOR HISTORY: Atleson, "Values and Assumptions in American Labor Law," 546; Barnard, "Walter Reuther and the Rise of the Auto Workers," 227; Conner, "The National War Labor Board," 865; Cooper, "The Army and Civil Disorder," 528; Cross, "Immigrant Workers in Industrial France," 784; Davies, "Woman's Place Is at the Typewriter," 1165; De Geer, "Job Studies and Industrial Relations," 158; De Shazo, "Urban Workers and Labor Unions in Chile, 1902-1927," 892; Debrizzi, "Ideology and the Rise of Labor Theory in America," 863; Ducker, "Men of the Steel Rails," 528; Engelstein, "Moscow, 1905," 490; Fink, "Workingmen's Democracy," 864; Galenson, "The United Brotherhood of Carpenters," 1400; Gorny, "The British Labour Movement and Zionism, 1917-1948," 441; Hoerder, editor "American Labor and Immigrant History, 1877-1920s," 529; Lichtenstein, "Labor's War at Home," 866; Light, "Working for the Railroad," 864; MacDowell, "Remember Kirkland Lake," 557; Moore, "Japanese Workers and the Struggle for Power, 1945-1947," 828; Moses, "Trade Unionism in Germany from Bismarck to Hitler, 1869-1933," 147; Papanikolas, "Buried Unsong," 229; Parpart, "Labor and Capital on the African Copperbelt," 1373; Prochaska, "History of the General Federation of Trade Unions, 1899-1980," 125; Rotella, "From Home to Office," 1165; Schnee, "Ben Tillett," 445; Schweider, "Black Diamonds," 229; Schöttler, "Die Entstehung der 'Bourses du travail,'" 456; Sirianni, "Workers Control and Socialist Democracy," 1122; Struthers, "No Fault of Their Own," 557; Takaki, "Pau Hana," 857; Tebbutt, "Making Ends Meet," 433; Tiutiukin, "Pervaia rossiiskaia revoliutsia i G. V. Plekhanov," 172; Waller, "The Dukeries Transformed," 1330; Weisser, "April 10," 430
 "Labor in a New Land," by Innes, 840
 "Labor's War at Home," by Lichtenstein, 866
 LaCapra, Dominick, "Rethinking Intellectual History: Texts, Contexts, Language," 1048
 Ladero Quesada, Miguel Ángel, "El siglo XV en Castilla: Fuentes de renta y política fiscal," 1088
 Lagemann, Ellen Condcliffe, "Private Power for the Public Good: A History of the Carnegie Foundation for the Advancement of Teaching," 551
 Lagemann, Ellen Condcliffe, editor, "Nursing History: New Perspectives, New Possibilities" (E), 571
 Lam, Truong Buu (R), 834
 Lamartine, Alphonse de, 1085
 Lamberti, Marjorie (R), 143
 Lambton, Ann K. S., "State and Government in Medieval Islam: An Introduction to the Study of Islamic Political Theory; The Jurists," 1125
 Lamirande, Emilien (R), 1316
 Lampe, John R. (R), 805
 "Lancastrian Normandy, 1415-1450," by Allmand, 1324
 "Land, Family, and Inheritance in Transition," by Howell, 1070
 "Land Reform in China and North Vietnam," by Moise, 184
 Landes, David S., "Revolution in Time: Clocks and the Making of the Modern World," 1052
 "Landesbeschreibungen Mitteleuropas vom 15. bis 17. Jahrhundert," edited by Harder (E), 1429
 "Landlord and Tenant in Urban Britain, 1838-1918," by Englander, 769
 Lane, Ann J. (R), 205
 Langley, Lester D., "The Banana Wars: An Inner History of American Empire, 1900-1934," 882
 LANGUAGE AND LINGUISTICS: Durkacz, "The Decline of the Celtic Languages," 127; Paulson, "Language, Science, and Action," 1308; Stock, "The Implications of Literacy," 107
 "Language, Science, and Action," by Paulson, 1308
 "Large Estates and Small Holdings in Europe in the Middle Ages and Modern Times," by Gunst and Hoffmann (E), 243
 Larson, Gary O., "The Reluctant Patron: The United States Government and the Arts, 1943-1965," 552
 Laskier, Michael M., "The Alliance Israélite Universelle and the Jewish Communities of Morocco: 1862-1962," 1371
 Laslett, J. H. M. (R), 863
 "Late Roman Fortifications," by Johnson, 743
 "Latin American Populism in Comparative Perspective," edited by Conniff, 564
 "The Latvian Impact on the Bolshevik Revolution," by Ezergailis, 1121
 "Latvijas Vēsture, 1920-1940," by Anderson, 1358
 "Laurence Oliphant, 1829-1888," by Taylor, 121
 Laurent, Pierre-Henri (R), 1342
 Laurie, Bruce (R), 1157
 "Law and Revolution," by Berman, 1040
 "Law School," by Stevens, 1398
 Lawlor, Sheila, "Britain and Ireland, 1914-23," 1082
 "Lay Authority and Reformation in the English Church," by Rodes, 760
 Lazerson, Marvin (R), 1176
 Lears, T. J. Jackson, and Richard Wightman Fox, editors, "The Culture of Consumption: Critical Essays in American History, 1880-1980," 1172
 Leavitt, Judith Walzer (R), 532
 Lebra, Joyce C. (R), 1137
 Leclercq, Jean, "Le mariage vu par les moines au XII^e siècle," 1062
 Lee, J. Bracken, 226
 Lee, Loyd E. (R), 1100
 Lee, Maurice, Jr. (R), 776; 1326
 Lees, Andrew (R), 142
 Leffler, Melvyn P., *The American Conception of National Security and the Beginnings of the Cold War, 1945-48* (Forum), 346-81
 Leffler, Melvyn P., *Reply to Comments on Leffler, The American Conception of National Security and the Beginnings of the Cold War, 1945-48* (Forum), 391-400
 "The Legacy of George Mason," edited by Pacheco, 1389
 "The Legacy of Jewish Migration," edited by Berger (E), 895
 "The Legacy of the Bolshevik Revolution," vol. 1 of "A Critical History of the USSR," by Rousset, 495

- LEGAL HISTORY AND THE LAW: Amelang, *Barristers and Judges in Early Modern Barcelona: The Rise of a Legal Elite*, 1264–84; Auerbach, "Justice without Law?" 533; Bakken, "The Development of Law on the Rocky Mountain Frontier," 855; Barnes, "Journey from Jim Crow," 1411; Berman, "Law and Revolution," 1040; Boari, "Qui venit contra iura," 1105; Borah, "Justice by Insurance," 1418; Chase, "The American Law School and the Rise of Administrative Government," 875; Cohen, "Theft in Athenian Law," 1057; Emel'ianova, "'Vseobshchaia istoriia prava' v russkom dorevoliutsionnom pravovedenii, XIX vek," 488; Hanley, "The *Lit de Justice* of the Kings of France," 1333; Hannig, "Consensus fidelium," 419; Howell, "The Judicial Committee of the Privy Council, 1833–1876," 437; Irons, "Justice at War," 871; Ives, "The Common Lawyers of Pre-Reformation England," 756; Kristensen, "Tacitus' germanische Gefolgschaft," 743; Lambton, "State and Government in Medieval Islam," 1125; Martin, "The Hypocrisy of Justice in the Belle Époque," 1337; Matthews, "Rufus Choate," 1155; Raban, "Mortmain Legislation and the English Church, 1279–1500," 416; Raeff, "The Well-Ordered Police State," 404; Scharer, "Die angelsächsische Königsurkunde im 7. und 8. Jahrhundert," 1063; Westphall, "Mercedes Reales," 1156
- Lehmann, Hartmut, and Dieter Lohmeier, editors, "Aufklärung und Pietismus im dänischen Gesamtstaat, 1770–1820" (E), 1199
- Lelyveld, David (R), 505
- Lemarchand, René (R), 821
- "Lenin and the Problem of Marxist Peasant Revolution," by Kingston-Mann, 1120
- Leonhard, Joachim-Felix, "Die Seestadt Ancona im Spätmittelalter: Politik und Handel," 1323
- Leopold, Joan (R), 410
- Lerner, Robert E., "The Powers of Prophecy: The Cedar of Lebanon Vision from the Mongol Onslaught to the Dawn of the Enlightenment," 746
- Lesch, Ann M. (R), 497; 1366
- "Let 'Em Holler," by Lythgoe, 226
- "The Letters of Henry Adams, 1858–1892," edited by Levenson *et al.*, 219
- Leuchtenburg, William E., "In the Shadow of FDR: From Harry Truman to Ronald Reagan," 1185
- Levenson, J. C., *et al.*, editors, "The Letters of Henry Adams, 1858–1892," 219
- Levi, Steven C., "Committee of Vigilance: The San Francisco Chamber of Commerce Law and Order Committee, 1916–1919; A Case Study in Official Hysteria," 230
- Levillain, Philippe, "Albert de Mun: Catholicisme français et catholicisme romain du *Syllabus* au *Ralliement*," 455
- Levine, Daniel H. (R), 1193
- Levine, David, *et al.*, "Essays on the Family and Historical Change" (E), 895
- Levine, Lawrence W., *William Shakespeare and the American People: A Study in Cultural Transformation*, 34–66
- Levy, Jack S., "War in the Modern Great Power System, 1495–1975," 1046
- Lewis, Bernard (C), 587
- Lewis, Bernard, and Benjamin Braude, editors, "Christians and Jews in the Ottoman Empire: The Functioning of a Plural Society," vol. 1, "The Central Lands"; vol. 2, "The Arabic-Speaking Lands" (E), 1199
- Lewis, Gordon K. (R), 1423
- Lewis, Gordon K., "Main Currents in Caribbean Thought: The Historical Evolution of Caribbean Society in Its Ideological Aspects, 1492–1900," 887
- Lewis, Jan, "The Pursuit of Happiness: Family and Values in Jefferson's Virginia," 1148
- Lewis, Naphtali, "Life in Egypt under Roman Rule," 1058
- "The Liberal Opposition in Hungary, 1919–1945," by Nagy, 1112
- "Liberal Party Politics," edited by Bogdanor (E), 246
- LIBERALISM: Johnpoll, "The Impossible Dream," 191; Nagy, "The Liberal Opposition in Hungary, 1919–1945," 1112; Schieder, editor, "Liberalismus in der Gesellschaft des deutschen Vormärz," 1348; Seidman, "Liberalism and the Origins of European Social Theory," 1297
- "Liberalism and the Origins of European Social Theory," by Seidman, 1297
- "Liberalismus in der Gesellschaft des deutschen Vormärz," edited by Schieder, 1348
- "Liberation Deferred?" by Bacchi, 236
- "The Liberty We Seek," by Potter, 513
- LIBRARIES AND ARCHIVES: Berner, "Archival Theory and Practice in the United States," 837
- Lichtenstein, Nelson, "Labor's War at Home: The CIO in World War II," 866
- Liebman, Ellen, "California Farmland: A History of Large Agricultural Landholdings," 856
- Lieven, D. C. B., "Russia and the Origins of the First World War," 1362
- "Life in Egypt under Roman Rule," by Lewis, 1058
- "The Life of Herbert Hoover," by Nash, 545
- Light, Walter, "Working for the Railroad: The Organization of Work in the Nineteenth Century," 864
- Lightman, Allan J. (R), 867
- Lightner, David L. (R), 528; 864
- Lill, Rudolf, and Franco Valsecchi, editors, "Il nazionalismo in Italia e in Germania fino alla Prima guerra mondiale" (E), 1196
- Lin Biao, 500
- Lincoln, Abraham, 1395; 1432
- "The Lincoln Image," by Holzer *et al.*, 1395
- "The Lincoln Murder Conspiracies," by Hanchett, 1395
- Lindberg, Carter, "The Third Reformation? Charismatic Movements and the Lutheran Tradition," 1040
- Lindberg, David C. (R), 99
- Lindemann, Albert S. (R), 1041
- Lindemann, Albert S., "A History of European Socialism," 92
- Lindley, Richard B., "Haciendas and Economic Development: Guadalajara, Mexico, at Independence," 1189
- Lindner, Rudi Paul, "Nomads and Ottomans in Medieval Anatolia," 1124
- Linke, Horst Günther, "Das zarische Russland und der Erster Weltkrieg: Diplomatie und Kriegsziele, 1914–1917," 814
- Linz, Norbert, "Der Bund der Landwirte in der Ersten Tschechoslowakischen Republik: Struktur und Politik einer deutschen Partei in der Aufbauphase," 167
- Lippincott, Louise, "Selling Art in Georgian London: The Rise of Arthur Pond," 1327
- Lipton, David (R), 152
- Liscombe, R. Windsor (R), 123
- Liss, Peggy K., "Atlantic Empires: The Network of Trade and Revolution, 1713–1826," 92
- "The *Lit de Justice* of the Kings of France," by Hanley, 1333
- LITERATURE: Arellano, "Panorama de la literatura

- nicaragüense," 1190; Frantzen, "The Literature of Penance in Anglo-Saxon England," 417; Köpeczi, "Staatsräson und christliche Solidarität," 1111
- "Literature and History," edited by Schulze and Wetzles (E), 572
- "The Literature of Penance in Anglo-Saxon England," by Frantzen, 417
- "The Little General and the Rousay Crofters," by Thomson, 448
- Littlefield, Daniel C., "Rice and Slaves: Ethnicity and the Slave Trade in Colonial South Carolina," 195
- "Littleton Waller Tazewell," by Peterson, 1394
- Livet, Georges, and Bernard Vogler, editors, "Pouvoir, ville et société en Europe, 1650–1750" (E), 567
- "Lloyd George and the Generals," by Woodward, 774
- Lloyd, Howell A., "The State, France, and the Sixteenth Century," 130
- Lloyd, Trevor (R), 772
- LOCAL HISTORY: Altschuler and Saltzgeber, "Revivalism, Social Conscience, and Community in the Burned-Over District," 204; Bartley, "The Creation of Modern Georgia," 1144; Bates, "Normandy before 1066," 113; Bennett, "Community, Class, and Careerism," 112; Campbell, "A Southern Community in Crisis," 1397; Collomp, "La maison du père," 1334; Cox, "Blacks in Topeka, Kansas, 1865–1915," 859; Davis, "Good and Faithful Labor," 521; Howell, "Land, Family, and Inheritance in Transition," 1070; Innes, "Labor in a New Land," 840; Kent and Kent, "Neighbours and Neighbourhood in Renaissance Florence," 1107; Kermod and Phillips, editors, "Seventeenth-Century Lancashire," 1326; Lukacs, "Philadelphia," 878; Madsen, "Gold Rush Sojourners in Great Salt Lake City, 1849 and 1850," 1393; Maier, "Priests and Power," 1372; McDonald and Wheeler, "Knoxville, Tennessee," 880; O'Connor, "A Sort of Utopia," 228; Prior, "Fisher Row," 429; Schüler, "Die Klostersäkularisation in Kent, 1535–1558," 759; Shifflett, "Patronage and Poverty in the Tobacco South," 521; Siaw, "Chinese Society in Rural Malaysia," 1140; Thomson, "The Little General and the Rousay Crofters," 448; Wayne, "The Reshaping of Plantation Society," 521; Weigley, editor, "Philadelphia," 878; Wynot, "Warsaw between the World Wars," 806
- Locke, John, 735
- Locke, Robert R. (R), 454
- Lockridge, Kenneth A. (R), 840
- Lockridge, Kenneth A., "The Fertility Transition in Sweden: A Preliminary Look at Smaller Geographic Units, 1855–1890," 1096
- Loetscher, Lefferts A., "Facing the Enlightenment and Pietism: Archibald Alexander and the Founding of Princeton Theological Seminary," 202
- Loewenberg, Peter (C), 1224
- Loewenberg, Peter (R), 1301
- Loewenberg, Peter, "Decoding the Past: The Psychohistorical Approach," 94
- Logan, George M., "The Meaning of More's *Utopia*," 91
- Löhken, Henrik, "Ordines Dignitatum: Untersuchungen zur formalen Konstituierung der spätantiken Führungsschicht," 742
- Lohmeier, Dieter, and Hartmut Lehmann, editors, "Aufklärung und Pietismus im dänischen Gesamtstaat, 1770–1820" (E), 1199
- Loisy, Alfred, 1292
- Lomax, Derek W. (R), 1319
- "Lone Stars and State Gazettes," by Sibley, 524
- Long, David F., "Sailor-Diplomat: A Biography of James Biddle, 1783–1848," 849
- Long, James W. (R), 170
- Longworth, Philip (R), 807
- Lönnroth, Erik, 1094
- Loock, Hans-Dietrich, and Hagen Schulze, editors, "Parlamentarismus und Demokratie im Europa des 19. Jahrhunderts" (E), 242
- Lora, Ronald (R), 97
- "Lord Aberdeen," by Chamberlain, 768
- "Lord Reading," by Judd, 438
- Louis IX (St. Louis), 751; 1324
- Lourdau, W., and D. Verhelst, editors, "Benedictine Culture, 750–1050," 1318
- Loveday, Amos J., Jr., "The Rise and Decline of the American Cut Nail Industry: A Study of the Interrelationships of Technology, Business Organization, and Management Techniques," 1399
- Lovejoy, Paul E., "Transformations in Slavery: A History of Slavery in Africa," 820
- Lowe, Donald M. (R), 98
- Lowe, Donald M., "History of Bourgeois Perception," 1301
- Lowenthal, Abraham F., and Cynthia McClintock, editors, "The Peruvian Experiment Reconsidered," 891
- Lucas, Angela M., "Women in the Middle Ages: Religion, Marriage, and Letters," 747
- Lucey, Archbishop Robert E., 222
- "Lucien Bonaparte Maxwell," by Murphy, 209
- Lüdtke, Alf., "'Gemeinwohl,' Polizei und 'Festungspraxis': Staatliche Gewaltsamkeit und innere Verwaltung in Preussen, 1815–1850," 146
- Lukács, György, 482
- Lukacs, John, "Philadelphia: Patricians and Philistines, 1900–1950," 878
- "Lukács Reappraised," edited by Heller (E), 1196
- Lund, Niels, and Niels Skyum-Nielsen, editors, "Danish Medieval History: New Currents" (E), 249
- Lundgren, Burden S. (C), 588
- "The Lust for Power," by Jelinek, 1113
- Lustig, Mary Lou, "Robert Hunter, 1666–1734: New York's Augustan Statesman," 1387
- Luther, Martin, 463; 1098; 1346
- "Luther's Last Battles," by Edwards, 463
- Lutz, Heinrich, and Grete Klingenstein, editors, "Spezialforschung und 'Gesamtgeschichte': Beispiele und Methodenfragen zur Geschichte der frühen Neuzeit" (E), 246
- Lutz, Heinrich, and Helmut Rumpler, editors, "Österreich und die deutsche Frage im 19. und 20. Jahrhundert: Probleme der politisch-staatlichen und soziokulturellen Differenzierung im deutschen Mitteleuropa" (E), 898
- Lutz, Jessie G. (R), 500
- Lyon, John (R), 100
- Lyons, Paul (R), 1174
- Lythgoe, Dennis L., "Let 'Em Holler: A Political Biography of J. Bracken Lee," 226
- "Macartney of Lisanoure, 1737–1806," edited by Roebuck (E), 897
- MacCaffrey, Wallace T. (R), 423
- MacCormack, Sabine (R), 744
- MacDonagh, Oliver, "States of Mind: A Study of Anglo-Irish Conflict, 1780–1980," 778
- MacDonald, David (R), 1313
- MacDonald, J. Fred, "Blacks and White TV: Afro-Americans in Television since 1948," 555
- MacDonald, Michael (R), 426

- MacDowell, Laurel Sefton, "'Remember Kirkland Lake': The History and Effects of the Kirkland Lake Gold Miners' Strike, 1941-42," 557
- MacFarquhar, Roderick, "The Origins of the Cultural Revolution," vol. 2, "The Great Leap Forward, 1958-1960," 1136
- MacKenzie, David (R), 1359
- MacLeod, David I., "Building Character in the American Boy: The Boy Scouts, YMCA, and Their Forerunners, 1870-1920," 1169
- MacLeod, Murdo J. (R), 1418
- MacLeod, Murdo J., and Robert Wasserstrom, editors, "Spaniards and Indians in Southeastern Mesoamerica: Essays on the History of Ethnic Relations" (E), 1432
- MacLeod, R. C. (R), 1415
- MacMillan, David S. (R), 1142
- MacMullen, Ramsay (R), 741
- Maday, Bela C., and Marida Hollos, editors, "New Hungarian Peasants: An East Central European Experience with Collectivization" (E), 1200
- Madden, A. F., and W. H. Morris-Jones, editors, "Australia and Britain: Studies in a Changing Relationship" (E), 252
- Madsen, Brigham D., "Gold Rush Sojourners in Great Salt Lake City, 1849 and 1850," 1393
- Maffei, Domenico, and Paolo Nardi, editors, "Atti del simposio internazionale Cateriniano-Bernardiniano, Siena, 17-20 Aprile 1980" (E), 574
- "Mafia, Peasants, and Great Estates," by Arlacchi, 800
- Magner, Lois N. (R), 1072
- Magocsi, Paul Robert, "Galicia: A Historical Survey and Bibliographic Guide," 1115
- Mahon, John K., "History of the Militia and the National Guard," 1144
- Maier, D. J. E., "Priests and Power: The Case of the Dente Shrine in Nineteenth-Century Ghana," 1372
- "Main Currents in Caribbean Thought," by Lewis, 887
- Maiorini, Maria Grazia, "Il Mouvement Republicain Populaire partito della IV Repubblica," 1086
- "La maison du père," by Callomp, 1334
- Maizlish, Stephen E., "The Triumph of Sectionalism: The Transformation of Ohio Politics, 1844-1856," 1158
- Major, J. Russell (R), 131; 1296
- Major-Poetzel, Pamela, "Michel Foucault's Archaeology of Western Culture: Toward a New Science of History," 90
- "Making Ends Meet," by Tebbutt, 433
- "The Making of a Ruling Class," by Jenkins, 765
- "The Making of a Special Relationship," by Hunt, 232
- "The Making of a State," by Vann, 1347
- "The Making of Franklin D. Roosevelt," by Goldberg, 867
- Malcolm, Joyce L. (R), 1066
- Mallon, Florencia E., "The Defense of Community in Peru's Central Highlands: Peasant Struggle and Capitalist Transition, 1860-1940," 890
- Malone, Michael P., editor, "Historians and the American West" (E), 578
- Maltby, William S., "Alba: A Biography of Fernando Alvarez de Toledo, Third Duke of Alba, 1507-1582," 786
- "Man and Nature," edited by Emerson *et al.* (E), 570
- "Man and the Natural World," by Thomas, 733
- "Man of Wars," by Hankinson, 767
- "The Management of Scottish Society, 1707-1764," by Shaw, 776
- "Managers of Virtue," by Tyack and Hansot, 531
- Manning, Patrick (R), 823; 1371
- Manor, James, "Political Change in an Indian State: Mysore, 1917-1955," 506
- Manselli, Raoul and Josef Riedmann, editors, "Federico Barbarossa nel dibattito storiografico in Italia e in Germania," 114
- Mansingh, Surjit (R), 508
- Manuel, Frank E., "The Changing of the Gods," 1047
- Marcus, Edward, and Nathan Schmukler, editors, "Inflation through the Ages: Economic, Social, Psychological, and Historical Aspects" (E), 894
- Marcus, Harold G., "Ethiopia, Great Britain, and the United States, 1941-1974: The Politics of Empire," 1132
- Margerison, Kenneth, "P.-L. Roederer: Political Thought and Practice during the French Revolution," 452
- Maria Theresia, 250
- "Le mariage vu par les moines au XII^e siècle," by Leclercq, 1062
- Marín, Manuel Torres, "Chacabuco y Vergara: Sino y camino del Teniente General Rafael Maroto Ysern," 1192
- "La marine française et la Mer Noire, 1918-1919," by Masson, 1338
- Marks, John H. (R), 412
- Marks, Shula (R), 181
- Marks, Shula, and Peter Richardson, editors, "International Labour Migration: Historical Perspectives" (E), 1425
- Marr, David G. (R), 508
- "Marriage and Fertility in Chile," by McCaa, 563
- Marrone, Steven P., "William of Auvergne and Robert Grosseteste: New Ideas of Truth in the Early Thirteenth Century," 748
- Marsden, George (R), 861
- Marsh, Margaret S. (R), 212
- Marshall, J. D. (R), 429
- "The Marshall Plan Revisited," by Wexler, 1407
- Marsot, Afaf Lutfi Al-Sayyid (R), 178
- Martin, Albro (R), 216
- Martin, B. G. (R), 819
- Martin, Benjamin F. (R), 455
- Martin, Benjamin F., "The Hypocrisy of Justice in the Belle Époque," 1337
- Martin, Paul M., "L'Idée de royauté à Rome: De la Rome royale au consensus républicain," 413
- Martin, Philip L., and Alan Richards, editors, "Migration, Mechanization, and Agricultural Labor Markets in Egypt" (E), 1430
- Martin, R. H. (R), 743
- "Martin Van Buren," by Niven, 199
- Marvell, Andrew, 425
- Marvick, Elizabeth Wirth (R), 1307
- Marvick, Elizabeth Wirth, "The Young Richelieu: A Psychoanalytic Approach to Leadership," 1333
- "Marx as Politician," by Felix, 1304
- Marx, Karl, 1304
- "Marxismus und Industrielle Revolution," by Nolte, 1041
- "Marxist-Leninist 'Scientific Atheism' and the Study of Religion and Atheism in the USSR," by Thrower, 817
- "Marxistische deutsche Geschichtswissenschaft, 1917 bis 1933," by Kinner, 468
- Masek, Rosemary (R), 421
- Mason, David T., 1182
- Mason, George, 1389
- Masson, Philippe, "La marine française et la Mer Noire, 1918-1919," 1338
- "Materiály k politickým, hospodářským a sociálním dějinám Československa v letech 1929-1939," by Harna *et al.*, 482

- "The Mathematical Science of Christopher Wren," by Bennett, 118
- Mathews, Thomas G. (R), 889
- Matthaei, Julie (R), 547
- Matthaei, Julie A., "An Economic History of Women in America: Women's Work, the Sexual Division of Labor, and the Development of Capitalism," 215
- Matthews, Jean V., "Rufus Choate: The Law and Civic Virtue," 1155
- Matthews, R. C. O., *et al.*, "British Economic Growth, 1856–1973," 1077
- Maurras, Charles, 140
- Mavrogordatos, George Th., "Stillborn Republic: Social Coalitions and Party Strategies in Greece, 1922–1936," 162
- Mawdsley, Evan (R), 817; 1360
- Maxwell, Gordon S., and William S. Hanson, "Rome's North West Frontier: The Antonine Wall," 743
- Maxwell, Lucien Bonaparte, 209
- Maxwell, Robert S., and Robert D. Baker, "Sawdust Empire: The Texas Lumber Industry, 1830–1940," 527
- May, Elaine Tyler (R), 191
- Mayer, Thomas, "Egypt and the Palestine Question, 1936–1945," 178
- Maynes, Mary Jo (R), 790
- Mazlish, Bruce (R), 94
- Mazzini, Giuseppe, 1356
- "Mazzini: Tra insegnamento e ricerca," 1356
- McAlpin, Michelle Burge, "Subject to Famine: Food Crises and Economic Change in Western India, 1860–1920," 1139
- McCaa, Robert, "Marriage and Fertility in Chile: Demographic Turning Points in the Petorca Valley, 1840–1976," 563
- McCaffrey, Lawrence J. (R), 778
- McCagg, William (R), 737
- McCarthy, Mary, "Social Change and the Growth of British Power in the Gold Coast: The Fante States, 1807–1874," 1130
- McCarthy, Michael, "Dark Continent: Africa As Seen by Americans," 1369
- McCash, William B., "Thomas R. R. Cobb (1823–1862): The Making of a Southern Nationalist," 1392
- McChesney, R. D. (R), 499
- McClellan, Woodford (R), 92
- McClelland, Peter D., and Richard J. Zeckhauser, "Demographic Dimensions of the New Republic: American Interregional Migration, Vital Statistics, and Manumissions, 1800–1860," 201
- McClintock, Cynthia, and Abraham F. Lowenthal, editors, "The Peruvian Experiment Reconsidered," 891
- McCormick, E. Allen, editor, "Germans in America: Aspects of German-American Relations in the Nineteenth Century" (E), 900
- McCormick, Richard P. (R), 200
- McCoy, Donald R. (R), 837
- McCulloch, Samuel Clyde (R), 1143
- McDonald, Michael J., and William Bruce Wheeler, "Knoxville, Tennessee: Continuity and Change in an Appalachian City," 880
- McDonough, James Lee (R), 207
- McDougall, Mary Lynn (R), 121
- McDougall, Walter A. (R), 554
- McGiffert, Michael (C), 1217
- McGill, William J. (R), 159
- McGucken, William (R), 1163
- McGuire, J. I., and Art Cosgrove, editors, "Parliament and Community" (E), 1198
- McHardy, A. K. (R), 416
- McIntire, C. T., "England against the Papacy, 1858–1861: Tories, Liberals, and the Overthrow of Papal Temporal Power during the Italian Risorgimento," 1068
- McKay, Derek, and H. M. Scott, "The Rise of the Great Powers, 1648–1815," 405
- McKinzie, Richard D. (R), 552
- McKitterick, Rosamond, "The Frankish Kingdoms under the Carolingians, 751–987," 1317
- McLaren, Angus, "Sexuality and Social Order: The Debate over the Fertility of Women and Workers in France, 1770–1920," 781
- McLaurin, Melton A. (R), 864
- McLean, David, "Timothy Pickering and the Age of the American Revolution," 514
- McLoughlin, William G., "Cherokees and Missionaries, 1789–1839," 1390
- McMurry, Richard M. (R), 852
- McMurry, Richard M., "John Bell Hood and the War for Southern Independence," 844
- McNamara, Jo Ann, "A New Song: Celibate Women in the First Three Christian Centuries," 1316
- "The McNeil Century," by Keve, 1166
- McNeill, William H. (R), 736
- McSeveney, Samuel T. (R), 530
- McTague, John J., "British Policy in Palestine, 1917–1922," 1366
- McWhinney, Edward, "Canada and the Constitution, 1979–1982: Patriation and the Charter of Rights," 559
- McWilliams, Tennant (R), 211
- "The Meaning of More's *Utopia*," by Logan, 91
- MEDICINE AND MEDICAL HISTORY: Armstrong, "Political Anatomy of the Body," 445; Dain, "Clifford W. Beers," 548; Gottfried, "The Black Death," 1322; Grob, "Mental Illness and American Society, 1875–1940," 1167; Hamilton, "The Healers," 128; Hopkins, "Princes and Peasants," 1049; Starr, "The Social Transformation of American Medicine," 532
- "Medieval Historical Writing in the Christian and Islamic Worlds," edited by Morgan (E), 243
- "A Mediterranean Society," vol. 4, "Daily Life," by Goitein, 1365
- Meier, August, and Elliott Rudwick, *J. Franklin Jameson, Carter G. Woodson, and the Foundations of Black Historiography* (Research Note), 1005–15
- Meikle, Jeffrey L. (R), 874
- Meilink-Roelofs, M. A. P., editor, "Dutch Authors on West Indian History: A Historiographical Selection" (E), 254
- Meiners, Fredericka, "A History of Rice University: The Institute Years, 1907–1963," 881
- "Melaka," edited by Sandhu and Wheatley, 835
- Melosh, Barbara (R), 1165
- "Men of the Steel Rails," by Ducker, 528
- Mendels, Franklin (R), 797; 1344
- Mendelsohn, Ezra, "The Jews of East Central Europe between the World Wars," 737
- "Mental Illness and American Society, 1875–1940," by Grob, 1167
- "Mercedes Reales," by Westphall, 1156
- "La Merindad de Estella en la edad moderna," by Floristán Imízcoz, 1088
- Mering, John V. (R), 849
- Merkel, Peter H. (R), 796
- Merl, Stephan, "Der Agrarmarkt und die Neue Ökonomische Politik: Die Anfänge staatlicher Lenkung der Landwirtschaft in der Sowjetunion, 1925–1928," 494
- Metcalf, Barbara D. (R), 1378
- Metcalf, George R., "From Little Rock to Boston: The History of School Desegregation," 551

- Metcalf, Michael F. (R), 461
 Metcalf, Thomas R. (R), 504
 "Metropolis and Province," edited by Inkster and Morrell (E), 245
 "Metropolis, 1890–1940," edited by Sutcliffe (E), 1426
 "Metropolitan Corridor," by Stilgoe, 1174
 "Michel Foucault's Archaeology of Western Culture," by Major-Poetzel, 90
 "The Microfoundations of Macrosociology," edited by Hechter (E), 571
 "The Middle East and North Africa," edited by Rosof *et al.* (E), 243
 Middleton, Charles R. (R), 768; 1328
 "Migration, Mechanization, and Agricultural Labor Markets in Egypt," edited by Richards and Martin (E), 1430
 Mikalson, Jon D., "Athenian Popular Religion," 1057
 Miles, Edwin A. (R), 525
 Miliband, Ralph, "Capitalist Democracy in Britain," 438
 MILITARY AND NAVAL HISTORY: Baily, "Faint Praise," 869; Ball, "Politics and Force Levels," 554; Ballard, "The Shock of Peace," 233; Browning, "Two if by Sea," 1154; Brun, "Eisphora-Syntaxis-Stratitika," 1312; Cole, "Gunboats and Marines," 538; Erickson, "The Road to Berlin," 1122; Foerster *et al.*, "Anfänge westdeutscher Sicherheitspolitik, 1945–1956," vol. 1, "Von der Kapitulation bis zum Plevien-Plan," 472; Hanson and Maxwell, "Rome's North West Frontier," 743; Hargrove, "General John Burgoyne," 428; Hattaway and Jones, "How the North Won," 207; Holley, "General John M. Palmer, Citizen Soldiers, and the Army of a Democracy," 225; Johnson, "Far China Station," 517; Johnson, "Late Roman Fortifications," 743; Langley, "The Banana Wars," 882; Mahon, "History of the Militia and the National Guard," 1144; Marín, "Chacabuco y Vergara," 1192; Masson, "La marine française et la Mer Noire, 1918–1919," 1338; Nunn, "Yesterday's Soldiers," 239; Proctor, "Hitler's Luftwaffe in the Spanish Civil War," 1352; Shea, "The Virginia Militia in the Seventeenth Century," 1145; Spector, "Advice and Support," 1411; Spiers, "Radical General," 1071; Stagg, "Mr. Madison's War," 1153; Stanley, "The War of 1812," 1153; Still, "American Sea Power in the Old World," 517; Sullivan, "Thomas-Robert Bugeaud," 133; Y'Blood, "Hunter-Killer," 1406
 "The Military and Society," edited by Rosof *et al.* (E), 242
 "Mill and Town in South Carolina, 1880–1920," by Carlton, 210
 Millar, James R. (R), 1361
 "Millenarianism and Peasant Politics in Vietnam," by Ho Tai, 508
 Miller, David Harry (R), 1064
 Miller, David W. (R), 449
 Miller, Marion S. (R), 1356
 Miller, R. F., and F. Fehér, editors, "Krushchev and the Communist World" (E), 1429
 Miller, Randall M. (R), 555
 Miller, Randall M., and Jon L. Wakelyn, editors, "Catholics in the Old South: Essays on Church and Culture," 850
 Miller, Sally M. (R), 1164
 Mills, Dennis (R), 429
 Millsaugh, Abbott (C), 585
 Milton, Joyce, and Ronald Radosh, "The Rosenberg File: A Search for the Truth," 873
 Minault, Gail, editor, "The Extended Family: Women and Political Participation in India and Pakistan," 833
 Miner, H. Craig, "The Rebirth of the Missouri Pacific, 1956–1983," 1410
 Ming-Le, Yao, "The Conspiracy and Death of Lin Biao," 500
 "The Minister's Wife," by Sweet, 516
 "Les ministres de la République, 1871–1914," by Estèbe, 138
 Mints, I. I., "God 1918i," 1360
 Mintz, Steven, "A Prison of Expectations: The Family in Victorian Culture," 431
 "Miracle in the Early Christian World," by Kee, 1292
 "Miracles and Prophecies in Nineteenth-Century France," by Kselman, 782
 Mirak, Robert, "Torn between Two Lands: Armenians in America, 1890 to World War I," 1401
 Misiunas, Romuald J., and Rein Taagepera, "The Baltic States: Years of Dependence, 1940–1980," 807
 "Mission on the Rhine," by Tent, 154
 "The Missionary Mind and American East Asia Policy, 1911–1915," by Reed, 537
 "Mr. Madison's War," by Stagg, 1153
 Mitchell, Harvey (R), 780
 Mitchell, Richard H., "Censorship in Imperial Japan," 827
 Mizruchi, Ephraim H., "Regulating Society: Marginality and Social Control in Historical Perspective," 410
 "Mobility and Integration in Urban Argentina," by Szuchman, 1194
 Moch, Leslie Page, "Paths to the City: Regional Migration in Nineteenth-Century France," 782
 MODERNIZATION: Whisnant, "Modernizing the Mountaineer," 231
 "Modernizing the Mountaineer," by Whisnant, 231
 "Mohammed, Charlemagne, and the Origins of Europe," by Hodges and Whitehouse, 1061
 Mohl, Raymond A. (R), 228
 Moise, Edwin E. (R), 185
 Moise, Edwin E., "Land Reform in China and North Vietnam: Consolidating the Revolution at the Village Level," 184
 Molin, Karl, "Hemmakriget: Om den svenska Krigsmaktens åtgärder mot kommunister under andra världskriget," 156
 Mollat, M., *et al.*, "L'Importance de l'exploration maritime au siècle des lumières: À propos du voyage de Bougainville" (E), 246
 Mollenkopf, John H., "The Contested City," 1175
 Molyneux, William, 129
 Momigliano, Arnaldo (R), 105
 Mommsen, Wolfgang J., and Lothar Kettenacker, editors, "The Fascist Challenge and the Policy of Appeasement" (E), 568
 Monaghan, E. Jennifer, "A Common Heritage: Noah Webster's Blue-Back Speller," 202
 MONASTICISM: Lederer, "Le mariage vu par les moines au XII^e siècle," 1062; Lourdaux and Verhelst, editors, "Benedictine Culture, 750–1050," 1318; McNamara, "A New Song," 1316
 Money, John (R), 765
 "La monnaie en France au début du XIX^e siècle," by Thuillier, 1335
 "Monopolists and Freebooters," vol. 2 of "The Pacific since Magellan," by Spate, 734
 Montesquieu, baron de La Brée et de (Charles de Secondat), 451; 1084
 "The Montgolfier Brothers and the Invention of Aviation, 1783–1784," by Gillispie, 1052
 Montgolfier, Etienne, 1052
 Montgolfier, Joseph, 1052

- Moore, Barrington, Jr., 403
 Moore, Joe, "Japanese Workers and the Struggle for Power, 1945-1947," 828
 Moore, John H., "Growth with Self-Management: Yugoslav Industrialization, 1952-1975," 805
 Moore, R. J., "Escape from Empire: The Attlee Government and the Indian Problem," 439
 Moore, Winfred B., Jr., and Walter J. Fraser, Jr., editors, "The Southern Enigma: Essays on Race, Class, and Folk Culture" (E), 1201
 "Moral Order and the Question of Change," edited by Wyatt and Woodside (E), 251
 Morales Carrión, Arturo, *et al.*, "Puerto Rico: A Political and Cultural History," 889
 Mordek, Hubert, editor, "Aus Kirche und Reich: Studien zu Theologie, Politik und Recht im Mittelalter; Festschrift für Friedrich Kempf zu seinem fünfundsiebzigsten Geburtstag und fünfzigjährigen Doktorjubiläum" (E), 1427
 More, Sir Thomas, 91; 422
 Morgan, D. O., editor, "Medieval Historical Writing in the Christian and Islamic Worlds" (E), 243
 Morgan, David W. (R), 147
 Morgan, H. Wayne (R), 1162
 Morgan, Kenneth O. (R), 774
 Mori, Giorgio, and Peter Hertner, editors, "La transizione dall'economia di guerra all'economia di pace in Italia e in Germania dopo la Prima guerra mondiale" (E), 575
 "Morpeth," by Olien, 432
 Morrell, Jack, and Ian Inkster, editors, "Metropolis and Province: Science in British Culture, 1780-1850" (E), 245
 Morris, Richard B. (R), 845
 Morris-Jones, W. H., and A. F. Madden, editors, "Australia and Britain: Studies in a Changing Relationship" (E), 252
 Morrissey, Will, "Reflections on de Gaulle: Political Founding in Modernity," 1340
 Morris, Roger, "The Royal Dockyards during the Revolutionary and Napoleonic Wars," 1328
 Morrissey, Thomas E. (R), 753
 Morse, Jedidiah, 515
 "La mort et l'Occident de 1300 à nos jours," by Vovelle, 1307
 "Mortmain Legislation and the English Church, 1279-1500," by Raban, 416
 Mosby, John Singleton, 852
 Mosca, Gaetano, 248
 "Moscow, 1905," by Engelstein, 490
 Moses, John A., "Trade Unionism in Germany from Bismarck to Hitler, 1869-1933," 147
 Mosher, Frederick C., "A Tale of Two Agencies: A Comparative Analysis of the General Accounting Office and the Office of Management and Budget," 1412
 "The Most Ancient Testimony," by Friedman, 1294
 "Mothers and Daughters," by Engel, 809
 Motz, Marilyn Ferris, "True Sisterhood: Michigan Women and Their Kin, 1820-1920," 1383
 Moulton, Edward C. (R), 1138
 "Il Mouvement Republicain Populaire partito della IV Repubblica," by Maiorini, 1086
 Moyer, Albert E., "American Physics in Transition: A History of Conceptual Change in the Late Nineteenth Century," 1163
 Moynihan, Ruth Barnes, "Rebel for Rights: Abigail Scott Duniway," 860
 Mozingo, David, and Victor Nee, editors, "State and Society in Contemporary China" (E), 577
 Mozarelli, Cesare, and Giuseppe Olmi, editors, "La corte nella cultura e nella storiografia: Immagini e posizioni tra otto e novecento" (E), 1429
 Mrozek, Donald J., "Sport and American Mentality, 1880-1910," 1168
 Muccigrosso, Robert (R), 1382
 Muir, Edward (R), 1109
 Muir, John, 223
 Muldowny, John (R), 231
 Mullaney, Marie Marmo, "Revolutionary Women: Gender and the Socialist Revolutionary Role," 1048
 Müller, Hans, "Die Kunst des Sklavenkaufs: Nach arabischen, persischen und türkischen Ratgebern vom 10. bis zum 18. Jahrhundert," 818
 Mulvey, Christopher, "Anglo-American Landscapes: A Study of Nineteenth-Century Anglo-American Travel Literature," 93
 Mundelein, Cardinal George, 221
 "El mundo rural sevillano en el siglo XV," by Fernández, 1088
 Munholland, Kim (R), 1338
 Murphy, Lawrence R., "Lucien Bonaparte Maxwell: Napoleon of the Southwest," 209
 Murphy, Richard Charles, "Guestworkers in the German Reich: A Polish Community in Wilhelmian Germany," 1351
 Musallam, B. F., "Sex and Society in Islam: Birth Control before the Nineteenth Century," 1363
 MUSIC: Starr, "Red and Hot," 174; Watson, "Song and Democratic Culture in Britain," 1081
 Muthesius, Stefan, "The English Terraced House," 768
 Myers, Ramon H. (R), 184
 "Mystic and Pilgrim," by Atkinson, 749
 "Nacionalismo Novohispano," by Bonett, 1188
 Nada, Narciso, "Guglielmo Moffa di Lisio, 1791-1877: Il contributo di un patriota braidese al Risorgimento nazionale," 1356
 Nafziger, E. Wayne, "The Economics of Political Instability: The Nigerian-Biafran War," 180
 Nagel, Paul C. (C), 589
 Nagl-Docekal, Herta, "Die Objektivität der Geschichtswissenschaft: Systematische Untersuchungen zum wissenschaftlichen Status der Historie," 1038
 Nagy, Zsuzsa L., "The Liberal Opposition in Hungary, 1919-1945," 1112
 Nahmanides, Rabbi Moses, 1196
 Naimark, Norman M., "Terrorists and Social Democrats: The Russian Revolutionary Movement under Alexander III," 489
 Naison, Mark, "Communists in Harlem during the Depression," 1174
 "Napoleon III and the Concert of Europe," by Echard, 453
 Nardi, Paolo, and Domenico Maffei, editors, "Atti del simposio internazionale Cateriniano-Bernardiniano, Siena, 17-20 Aprile 1980" (E), 574
 "Narrative Logic," by Ankersmit, 1037
 Nasatir, Abraham P., and Gilbert C. Din, "The Imperial Osages: Spanish-Indian Diplomacy in the Mississippi Valley," 1149
 Nasaw, David (R), 531
 Nash, Gary B. (R), 1146
 Nash, George H., "The Life of Herbert Hoover: The Engineer, 1874-1914," 545
 Nash, Michael, "Conflict and Accommodation: Coal Miners, Steel Workers, and Socialism, 1890-1920," 1164
 Nash, Roderick (R), 535

- Nashat, Guity (R), 1368
 Nashat, Guity, "The Origins of Modern Reform in Iran, 1870–80," 1367
 Naske, Claus-M. (R), 556
 "The National War Labor Board," by Conner, 865
 NATIONALISM: Bonett, "Nacionalismo Novohispano," 1188; Graus, "Die Nationenbildung der Westslawen im Mittelalter," 419; Henriksson, "The Tsar's Loyal Germans," 170; Jelinek, "The Lust for Power," 1113; Kaminsky and Kruk, "Le nationalisme arabe et le nationalisme juif," 1364; MacDonagh, "States of Mind," 778; O'Meara, "Volkskapitalisme," 1132; van Velthoven, "De Vlaamse Kwestie, 1830–1914," 1341; Walicki, "Philosophy and Romantic Nationalism," 484
 "Nationalism, Positivism, and Catholicism," by Sutton, 140
 "Le nationalisme arabe et le nationalisme juif," by Kaminsky and Kruk, 1364
 "Nationalismus in der Welt von Heute," edited by Winkler (E), 241
 "Die Nationenbildung der Westslawen im Mittelalter," by Graus, 419
 "Nature et valeur dans la philosophie de Montesquieu," by Beyer, 451
 "The Nature of History in Ancient Greece and Rome," by Fornara, 1311
 Nawyn, William E., "American Protestantism's Response to Germany's Jews and Refugees, 1933–1941," 872
 "The Nazi Movement in Baden, 1920–1945," by Grill, 471
 "The Nazi Party," by Kater, 469
 "Nazi Propaganda," edited by Welch (E), 899
 "Il nazionalismo in Italia e in Germania fino alla Prima guerra mondiale," edited by Lill and Valsecchi (E), 1196
 Neal, Steve, "Dark Horse: A Biography of Wendell Willkie," 1405
 Nee, Victor, and David Mazingo, editors, "State and Society in Contemporary China" (E), 577
 Neel, Carol (R), 1318
 "Négociants et fabricants dans l'industrie contonnière suisse, 1760–1840," by Veyrassat, 797
 "Negro Slavery in the Sugar Plantations of Veracruz and Pernambuco, 1550–1680," by Cardoso, 560
 "Neighbours and Neighbourhood in Renaissance Florence," by Kent and Kent, 1107
 "Neither Peace Nor War," by Egan, 198
 Nelli, Humbert S., "From Immigrants to Ethnic: The Italian Americans," 543
 Nenner, Howard (R), 1067
 "Neostoicism and the Early Modern State," by Oestreich, 403
 "A Nest of Hornets," by Reid, 1379
 "Networks of Power," by Hughes, 406
 Neu, Irene D. (R), 215
 Neuhaus, Helmut, "Reichsständische Repräsentationsformen im 16. Jahrhundert: Reichstag—Reichskreistag—Reichsdeputationstag," 463
 "The New Bourgeoisie and the Limits of Dependency," by Becker, 891
 "New England and Foreign Relations, 1789–1850," by Varg, 197
 "New Frontiers in American–East Asian Relations," edited by Cohen, 884
 "New Hungarian Peasants," edited by Hollos and Maday (E), 1200
 "New Jersey's Union College," by Raichle, 1408
 New, John F. H. (R), 761
 "The New Right, 1960–1968, with Epilogue, 1969–1980," by Kolkey, 1414
 "A New Song," by McNamara, 1316
 Newman, William J. (R), 1046
 Newton, Sir Isaac, 401
 Nicholas I, 810
 Nicholas, David (R), 114
 Nicols, John (R), 742
 Niedhart, Gottfried, editor, "Der Westen und die Sowjetunion: Einstellungen und Politik gegenüber der UdSSR in Europa und in den USA seit 1917" (E), 1197
 Niethammer, Lutz, editor, "'Die Jahre weiss man nicht, wo man die heute hinsetzen soll': Faschismuserfahrungen im Ruhrgebiet," 795
 Ninkovich, Frank (R), 1184
 Ninkovich, Frank A., "The Diplomacy of Ideas: U.S. Foreign Policy and Cultural Relations, 1938–1950," 883
 "The Ninth State," by Turner, 847
 Nipperdey, Thomas, "Deutsche Geschichte, 1800–1866: Bürgerwelt und starker Staat," 1099
 Nischan, Bodo (R), 1095
 Nissen, Henrik S., editor, "Scandinavia during the Second World War," 1092
 Niven, John, "Martin Van Buren: The Romantic Age of American Politics," 199
No Angels in the House: The Victorian Myth and the Paget Women, by Peterson, 677–708
 "No Fault of Their Own," by Struthers, 557
 Noakes, Jeremy (R), 471
 "The Nobility in Russia and Eastern Europe," edited by Banac and Bushkovitch, 807
 Noble, David W. (R), 1170
 "Noble Privilege," vol. 1 of "The European Nobility," by Bush, 1293
 Noble, Thomas F. X. (R), 419
 Nobles, Gregory H., "Divisions throughout the Whole: Politics and Society in Hampshire County, Massachusetts, 1740–1775," 1146
 Nocken, Ulrich (R), 149
 Nolte, Ernst, "Marxismus und Industrielle Revolution," 1041
 "Nomads and Ottomans in Medieval Anatolia," by Lindner, 1124
 "None is Too Many," by Abella and Troper, 886
 Noonan, Thomas S. (R), 745
 "Normandy before 1066," by Bates, 113
 "North Korea Today," edited by Scalapino and Kim (E), 577
 "Northern Ireland," edited by Darby (E), 897
 Norton, Mary Beth, *The Evolution of White Women's Experience in Early America*, 593–619
 "Nothing But Freedom," by Foner, 1160
 "The Notorious Triangle," by Coughtry, 841
 Nummela, Ilkka, and Mauno Jokipii, editors, "Ur Nordisk kulturhistoria: Den kritiska tanken i vetenskapen på 1700- och 1800-talen" (E), 249
 Nunn, Frederick M., "Yesterday's Soldiers: European Military Professionalism in South America, 1890–1940," 239
 "Nursing History," edited by Lagemann (E), 571
 Nwabara, S. N. (R), 180
 Nye, Robert A. (R), 90; 1337
 "O djelu Veselina Masleše," edited by Tanović *et al.* (E), 1200
 O'Boyle, Lenore (R), 123
 O'Brien, Charles H. (R), 1039
 O'Brien, David (R), 221

- O'Brien, Michael (R), 1171
 "Die Objektivität der Geschichtswissenschaft," by Nagl-Docekal, 1038
 "The Occult in America," edited by Kerr and Crow (E), 899
 Ocko, Jonathan K., "Bureaucratic Reform in Provincial China: Ting Jih-ch'ang in Restoration Kiangsu, 1867-1870," 182
 O'Connor, Carol A., "A Sort of Utopia: Scarsdale, 1891-1981," 228
 O'Day, Rosemary, "Education and Society, 1500-1800: The Social Foundations of Education in Early Modern Britain," 116
 Oestreich, Gerhard, "Neostoicism and the Early Modern State," 403
 Offen, Karen, *Depopulation, Nationalism, and Feminism in Fin-de-Siècle France*, 648-76
 "Officerarna och det svenska samhället, 1650-1700," by Asker, 1344
 "Ohivesa," by Wilson, 858
 "Oil and Politics in Latin America," by Philip, 1187
 "Oisiveté et loisirs dans les sociétés occidentales au XIX^e siècle," by Centre de recherche d'histoire sociale de l'université de picardie (E), 1198
 Okochi, Akio, and Tadakatsu Inoue, editors, "Overseas Business Activities" (E), 1426
 Olai, Birgitta, "Storskiftet i Ekebyborna: Svenskjordbruksutveckling avspeglad i en östgötasocken," 462
 "The Old Christian Right," by Ribuffo, 1185
 "The Old Country School," by Fuller, 531
 Olien, Diana Davids, "Morpeth: A Victorian Public Career," 432
 Oliphant, Laurence, 121
 Olmi, Giuseppe, and Cesare Mozzarelli, editors, "La corte nella cultura e nella storiografia: Immagini e posizioni tra otto e novecento" (E), 1429
 Olson, Richard, "Science Deified and Science Defied: The Historical Significance of Science in Western Culture from the Bronze Age to the Beginnings of the Modern Era ca. 3500 B.C. to ca. A.D. 1640," 99
 O'Meara, Dan, "Volkskapitalisme: Class, Capital, and Ideology in the Development of Afrikaner Nationalism, 1934-1948," 1132
 "On Making Use of History," edited by Zitomersky (E), 249
 Onorato, Michael Paul (R), 1140
 Onuf, Peter S. (R), 514
 Onuf, Peter S., "The Origins of the Federal Republic: Jurisdictional Controversies in the United States, 1775-1787," 846
 Oppenheim, Janet (R), 1081
 Oppenheim, Samuel A. (R), 1360
 ORAL HISTORY: Mulvey, "Anglo-American Landscapes," 93
 Orbach, Alexander (R), 810
 "Ordines Dignitatum," by Löhken, 742
 O'Reilly, Kenneth (R), 873
 O'Reilly, Kenneth, "Hoover and the Un-Americans: The FBI, HUAC, and the Red Menace," 873
 "Organizational Values and Political Power," by Twilight, 1181
 "Orient et occident au temps des croisades," by Cahen, 751
 "The Origins of Chinese Civilization," edited by Keightley (E), 252
 "Origins of Enterprise," by Honeyman, 435
 "Origins of Legislative Sovereignty and the Legislative State," vol. 1, "Corasius and the Renaissance Systematization of Roman Law"; vol. 2, "Classical, Medieval, and Renaissance Foundations of Corasius' Systematic Methodology," by Fell, 1296
 "The Origins of Modern Reform in Iran, 1870-80," by Nashat, 1367
 "The Origins of the Cultural Revolution," vol. 2, "The Great Leap Forward, 1958-1960," by MacFarquhar, 1136
 "The Origins of the Federal Republic," by Onuf, 846
 "The Origins of the Scottish Railway System, 1722-1844," by Robertson, 777
 Osheim, Duane J. (R), 115
 "Osmanli Devleti'ne Karsi Arap Bağımsizlik Hareketi, 1908-1918," by Kürkcüoğlu, 176
 "Österreich und die deutsche Frage im 19. und 20. Jahrhundert," edited by Lutz and Rumpel (E), 898
 "Die österreichischen Volksgruppen," by Suppan, 1104
 Ostwald, Martin (R), 1057
 "Other Tribes, Other Scribes," by Boon, 410
 Ott, Thomas (R), 887
 "The Ottoman Slave Trade and its Suppression, 1840-1890," by Toledano, 818
 "Our Gang," by Joselit, 1179
 "Out to Work," by Kessler-Harris, 193
 "Outremer," edited by Kedar *et al.*, 111
 "Overseas Business Activities," edited by Okochi and Inoue (E), 1426
 "Overseas Investment in the Age of High Imperialism," by Edelstein, 771
 Owen, Thomas C., "Capitalism and Politics in Russia: A Social History of the Moscow Merchants, 1855-1905," 1117
 Owings, W. A. (R), 804
 Ozment, Steven, "When Fathers Ruled: Family Life in Reformation Europe," 1295
 "P.-L. Roederer," by Margerison, 452
 Pacheco, Josephine F., editor, "The Legacy of George Mason," 1389
 "The Pacific since Magellan," vol. 2, "Monopolists and Freebooters," by Spate, 734
 "The Pacification of Melanesia," edited by Rodman and Cooper (E), 1201
 Packe, Michael, "King Edward III," 749
 Padberg, John W. (R), 1292
 Page, David, "Prelude to Partition: The Indian Muslims and the Imperial System of Control, 1920-1932," 505
 Page, Joseph A., "Perón: A Biography," 893
 PALEOGRAPHY AND TEXTUAL EXEGESIS: Jones, "Independence and Exegesis," 1292; Korshin, "Typologies in England, 1650-1820," 116
 "Palestine and the Great Powers, 1945-1948," by Cohen, 177
 Palliser, D. M., "The Age of Elizabeth: England under the Later Tudors, 1547-1603," 423
 Palmer, Alan, "The Chancelleries of Europe," 1045
 Palmer, John M., 225
 Pancake, John (R), 1387
 "Panorama de la literatura nicaragüense," by Arellano, 1190
 "Paolo Sarpi," by Wootton, 1109
 Papacostas, S. Victor (R), 478
 Papanikolas, Zeese, "Buried Unsung: Louis Tikas and the Ludlow Massacre," 229
 Papastratis, Procopis (R), 479
 "Papers and Reviews, 1973-1981," vol. 3 of "Studies in Tudor and Stuart Politics and Government," by Elton (E), 896
 "The Papers of George Washington, Colonial Series," vol. 1, "1748-August 1755"; vol. 2, "August 1755-April 1756," edited by Abbot *et al.*, 845

- Paracelsus, Philippus Aureolus, 401
 "Paradoxical Harvest," by Adams, 772
 Paret, Peter (R), 464
 "Pariahs Stand Up!" by Bidelman, 136
 "Paris City Councillors in the Sixteenth Century, by Diefendorf, 131
 Park, Yung Chul, and David C. Cole, "Financial Development in Korea, 1945–1978," 502
 Parks, J. D., "Culture, Conflict, and Coexistence: American-Soviet Cultural Relations, 1917–1958," 1123
 "Parlamentarismus und Demokratie im Europa des 19. Jahrhunderts," edited by Looock and Schulze (E), 242
 "O parlamento brasileiro e as relações exteriores, 1826–1889," by Cervo, 240
 "Parliament and Community," edited by Cosgrove and McGuire (E), 1198
 Parman, Donald L. (R), 871
 Parmet, Herbert S. (R), 1185
 Parmet, Robert D. (R), 529
 Parpart, Jane L., "Labor and Capital on the African Copperbelt," 1373
 Parrish, Michael (R), 1122
 Parry, Graham (R), 427
 "Parties and Politics in North Carolina, 1836–1865," by Kruman, 849
 "Partners in Revolution," by Elliott, 129
 Pasternak, Burton, "Guests in the Dragon: Social Demography of a Chinese District, 1895–1946," 1374
 "Pastor of the Poles," edited by Blejwas and Biskupski (E), 253
 Pastor, Peter (R), 1337
 Pastor, Peter, and Samuel R. Williamson, Jr., editors, "Essays on World War I: Origins and Prisoners of War" (E), 242
 Paterson, Hamish M., and Walter M. Humes, editors, "Scottish Culture and Scottish Education, 1800–1980" (E), 246
 "Paths to the City," by Moch, 782
 Patrick, Hugh (R), 1137
 "A Patron for Pure Science," by England, 102
 "Patronage and Poverty in the Tobacco South," by Shifflett, 521
 "Patronage in Late Renaissance England," by Fogle and Knafla (E), 572
 Patrucco, Armand (R), 802
 "Patterns in the Dust," by Tucker, 1180
 Patterson, Orlando, "Slavery and Social Death: A Comparative Study," 411
 Patterson, Robert B. (R), 111
 Patterson, Stephen E. (R), 197
 "Pau Hana," by Takaki, 857
 Paul, Arnold Milton (R), 875
 Paulson, Ross Evans, "Language, Science, and Action: Korzybski's General Semantics—A Study in Comparative Intellectual History," 1308
 "Les pauvres et le pouvoir," by Haesenne-Peremans, 1091
 Payne, Harry C. (R), 451
 Payne, Stanley G. (R), 1090
 "Peaceable Kingdom," by Harrison (E), 245
 Peale, Charles Willson, 847
 "Pearl Harbor, 7. Dezember 1941," by Herde, 540
 "Peasant China in Transition," by Shue, 185
 "Peasant Classes," by Rebel, 159
 "Peasant Movements in India, 1920–1950," by Dhanagare, 832
 Pease, Jane H. (R), 520
 "Peel, Priests, and Politics," by Kerr, 449
 Peel, Sir Robert, 449
 Pegg, Carl H., "Evolution of the European Idea, 1914–1932," 1303
 Pegues, Franklin J. (R), 1321
 Pejovich, Svetozar, editor, "Philosophical and Economic Foundations of Capitalism" (E), 241
 Pellicer, Olga, and Richard R. Fagen, editors, "The Future of Central America: Policy Choices for the U.S. and Mexico" (E), 901
 Pencak, William, "America's Burke: The Mind of Thomas Hutchinson," 194
 Pendleton, Gayle Trusdel (R), 770
 "The Penny Capitalists," by Benson, 1075
 "The People of the Cape Verde Islands," by Carreira, 179
 "Per la storia sociale e religiosa del Mezzogiorno d'Italia," edited by Galasso and Russo (E), 248
 "The Percys of Mississippi," by Baker, 1171
 "Pereiaslav 1654," by Basarab, 808
 Pérez, Louis A., Jr. (R), 1422
 Pérez, Louis A., Jr., "Cuba between Empires, 1878–1902," 1423
 Pérez Moreda, Vicente, "Las crisis de mortalidad en la España interior, siglos XVI–XIX," 458
 Perinbam, B. Marie, "Holy Violence: The Revolutionary Thought of Frantz Fanon; An Intellectual Biography," 1129
 Perkin, Harold (R), 443
 Perkins, Bradford (R), 198
 Pernal, A. B. (R), 483
 Perón, Juan, 893
 "Perón," by Page, 893
 Perrie, Maureen (R), 171
 Persell, Stuart M. (R), 1339
 Persell, Stuart Michael, "The French Colonial Lobby, 1889–1938," 1086
 "Le personnel de la chancellerie de Bourgogne-Flandre sous les ducs de Bourgogne de la maison de Valois, 1384–1477," by Cockshaw, 114
 "Perspectives on Kashmir," by Ishaq Khan, 1378
 "The Peruvian Experiment Reconsidered," edited by McClintock and Lowenthal, 891
 "Pervaia rossiiskaia revoliutsiia i G. V. Plekhanov," by Tiutiukin, 172
 "Pervaia rossiiskaia revoliutsiia i tsarizm," by Koroleva, 490
 "Pervoe serbskoe vosstanie 1804–1813 gg. i Rossiia," vol. 2, "1808–1813," edited by Dostian *et al.*, 1359
 Peters, Edward (R), 109
 Petersen, William (R), 1073
 Peterson, Edward N. (R), 154
 Peterson, M. Jeanne, *No Angels in the House: The Victorian Myth and the Paget Women*, 677–708
 Peterson, Merrill D. (R), 847
 Peterson, Norma Lois, "Littleton Waller Tazewell," 1394
 Petracchi, Giorgio, "La Russia rivoluzionaria nella politica italiana: Le relazioni italo-sovietiche, 1917–25," 477
 Petry, Carl F. (R), 1363
 Petulla, Joseph M. (R), 855
 Petzold, Joachim, "Die Demagogie des Hitlerfaschismus: Die politische Funktion der Naziideologie auf dem Wege zur faschistischen Diktatur," 151
 Pfau, Richard (R), 885
 Pflanze, Otto, editor, "Innenpolitische Probleme des Bismarck-Reiches" (E), 247
 "Philadelphia," by Lukacs, 878
 "Philadelphia," edited by Weigley, 878
 Philip II (of Macedon), 243
 "Philip II, Alexander the Great, and the Macedonian Heritage," edited by Adams and Borza (E), 243

- Philip, George, "Oil and Politics in Latin America: Nationalist Movements and State Companies," 1187
- Phillips, C. B., and J. I. Kermode, editors, "Seventeenth-Century Lancashire: Essays presented to J. J. Bagley," 1326
- Phillips, Joseph W., "Jedidiah Morse and New England Congregationalism," 515
- Phillips, Paul T., "The Sectarian Spirit: Sectarianism, Society, and Politics in Victorian Cotton Towns," 433
- "Philosophical and Economic Foundations of Capitalism," edited by Pejovich (E), 241
- "The Philosophical Naturalists," by Rehbock, 1072
- "Philosophy and Romantic Nationalism," by Walicki, 484
- PHILOSOPHY OF HISTORY: Ankersmit, "Narrative Logic," 1037; Beyer, "Nature et valeur dans la philosophie de Montesquieu," 451; Fell, "Origins of Legislative Sovereignty and the Legislative State," vol. 1, "Corasius and the Renaissance Systematization of Roman Law"; vol. 2, "Classical, Medieval, and Renaissance Foundations of Corasius' Systematic Methodology," 1296; Fernández-Santamaría, "Reason of State and Statecraft in Spanish Political Thought, 1595–1640," 457; Hakuseki, "Tokushi Yoron," 186; Hampson, "Will and Circumstance," 1084; Hartigan, "The Forgotten Victim," 96; LaCapra, "Rethinking Intellectual History," 1048; Lindemann, "A History of European Socialism," 92; Logan, "The Meaning of More's *Utopia*," 91; Marrone, "William of Auvergne and Robert Grosseteste," 748; Morrissey, "Reflections on de Gaulle," 1340; Oestreich, "Neostoicism and the Early Modern State," 403; Pegg, "Evolution of the European Idea, 1914–1932," 1303; Quigley, "Weapons Systems and Political Stability," 98; Schmitt, "Aristotle and the Renaissance," 755; Schmitt, "John Case and Aristotelianism in Renaissance England," 757; Wood, "The Politics of Locke's Philosophy," 735
- PHOTOGRAPHY: Ford and Harrison, "A Hundred Years Ago," 434; Ginsburg, "Victorian Dress in Photographs," 434
- PHYSICAL SCIENCES: Clements, "William Jennings Bryan," 220; Cohen, "Political Parties in the West Bank under the Jordanian Regime, 1949–1967," 176; Greene, "Geology in the Nineteenth Century," 101; Webster, "From Paracelsus to Newton," 401
- "Il Piano Marshall e l'Europa," edited by Rossi (E), 1199
- Pichardo, Hortensia, "Biografía del Colegio de San Cristóbal de La Habana," 1422
- Pickering, Timothy, 514
- Piedisicalzi, Nicholas, and David L. Barr, editors, "The Bible in American Education: From Source Book to Textbook" (E), 252
- Pierson, Peter (R), 786
- Pierson, Stanley (R), 770
- Piggott, Stuart, "The Earliest Wheeled Transport: From the Atlantic Coast to the Caspian Sea," 738
- Pike, Ruth (R), 459; 1088
- Pinkney, Alfonso (R), 230
- "Pioneer Plastic," by Friedel, 1183
- Pipes, Daniel (C), 587
- Pirotte, Jean, editor, "Stéréotypes nationaux et préjugés raciaux aux XIX^e et XX^e siècles: Sources et méthodes pour une approche historique" (E), 241
- Pitt, William, 766
- Plakans, Andrejs (R), 1358
- "The Plantation Mistress," by Clinton, 196
- Platt, D. C. M., "Foreign Finance in Continental Europe and the United States, 1815–1870: Quantities, Origins, Functions, and Distribution," 1302
- Platt, Harold L., "City Building in the New South: The Growth of Public Services in Houston, Texas, 1830–1910," 209
- "Plehve," by Judge, 812
- Plehve, Viacheslav Konstantinovich, 812
- Plekhanov, G. V., 172
- Plessis, Alain, "La Banque de France et ses deux cents actionnaires sous le Second Empire," 134
- Pletcher, David M. (R), 541
- "La población de Luisiana Española, 1763–1803," by Acosta Rodríguez, 843
- Pocock, Emil, *Presidents of the American Historical Association: A Statistical Analysis* (Research Note), 1016–24
- "Podatnaia reforma Petra I," by Anisimov, 1117
- Poesch, Jessie, "The Art of the Old South: Painting, Sculpture, Architecture, and the Products of Craftsmen," 1382
- "The Poet's Time," by Chernaik, 425
- Pole, J. R., "The Gift of Government: Political Responsibility from the English Restoration to American Independence," 735
- "Polen und Deutschland im Zeitalter der Aufklärung," edited by Jeismann and Riemenschneider (E), 247
- "Policing a Class Society," by Harring, 1401
- "Political Anatomy of the Body," by Armstrong, 445
- "Political Change in an Indian State," by Manor, 506
- POLITICAL HISTORY: Alderman, "The Jewish Community in British Politics," 440; Allmand, "Lancastrian Normandy, 1415–1450," 1324; Andaya, "The Heritage of Arung Palakka," 510; Baker, "Affairs of Party," 200; Baker, *The Domestication of Politics: Women and American Political Society, 1780–1920*, 620–47; Balfour, "West Germany," 156; Bartley, "The Creation of Modern Georgia," 1144; Barudio, "Gustav Adolf—der Grosse," 1095; Becker, "The New Bourgeoisie and the Limits of Dependency," 891; Bergeron, "Antebellum Politics in Tennessee," 525; Blinov, "Tsentral'nyi sovet fabzavkomov Petrograda, 1917–1918 gg.," 817; Bowen, "Rebellion and Democracy in Meiji Japan," 187; Bowman, "De Valera and the Ulster Question, 1917–1973," 449; Byrnes, editor, "After Brezhnev," 496; Cahen, "Orient et occident au temps des croisades," 751; Cervo, "O parlamento brasileiro e as relações exteriores, 1826–1889," 240; Chamberlain, "Lord Aberdeen," 768; Charlot, "Le gaullisme d'opposition, 1946–1958," 784; Chernaik, "The Poet's Time," 425; Chubb, "The Government and Politics of Ireland," 777; Collins, "Shadows in the Grass," 824; Cooper, "The Warrior and the Priest," 1170; Coward, "The Stanleys," 1071; Davies, "Wales in the Early Middle Ages," 416; Dembkowski, "The Union of Lublin," 483; Dinkin, "Voting in Revolutionary America," 197; Dragnich, "The First Yugoslavia," 480; Echard, "Napoleon III and the Concert of Europe," 453; Eldot, "Governor Alfred E. Smith," 867; Feldbæk, "Danmarks historie," vol. 4, "Tiden 1730–1814," 461; Fennell, "The Crisis of Medieval Russia, 1200–1304," 487; Ferenczi, "Aussenpolitik und Öffentlichkeit in Russland, 1906–1912," 491; Fink, "Workingmen's Democracy," 864; Finlayson, "Historians, Puritanism, and the English Revolution," 1066; Fitz, "L'Administration des provinces panoniennes sous le Bas-Empire romain," 745; Fitzpatrick, "Catholic Royalism in the Department of the Gard,

- 1814–1852," 783; Formisano, "The Transformation of Political Culture," 200; Fortescue, "Alphonse de Lamartine," 1085; Foster, "The House of Lords, 1603–1649," 1067; Getzler, "Kronstadt, 1917–1921," 492; Goldstein, "Political Repression in 19th-Century Europe," 1044; Grant, "From Alexander to Cleopatra," 103; Hagen, "Die Entfaltung politischer Öffentlichkeit in Russland, 1906–1914," 491; Hammack, "Power and Society," 213; Hanchett, "The Lincoln Murder Conspiracies," 1395; Harna *et al.*, "Materiály k politickým, hospodářským a sociálním dějinám Československa v letech 1929–1939," 482; Harris, "Clarendon and the English Revolution," 1326; Hatzenbuehler and Ivie, "Congress Declares War," 1391; Heinemann, "Depression and New Deal in Virginia," 868; Heinemann, "Die verdrängte Niederlage," 1352; Heinig, "Reichsstädte, freie Städte und Königtum, 1389–1450," 1345; Hietanen, "Siirtoväen Pika-Asutuslaki 1940," 158; Holm, "Theodosian Empresses," 415; Hunt, "The Puritan Moment," 117; Jones, "Faith by Statute," 1325; Jordan, "Political Leadership in Jefferson's Virginia," 1152; Jouve, "L'Âge d'or de la caricature anglaise," 770; Judge, "Plehve," 812; Kaminsky, "Simon de Cramaud and the Great Schism," 753; Kedar *et al.*, editors, "Outremer," 111; Keefe, "Feudal Assessments and the Political Community under Henry II and His Sons," 1320; Kingston-Mann, "Lenin and the Problem of Marxist Peasant Revolution," 1120; Kofas, "Authoritarianism in Greece," 802; Kraut, editor, "Crusaders and Compromisers," 1158; Kruman, "Parties and Politics in North Carolina, 1836–1865," 849; Lawlor, "Britain and Ireland, 1914–23," 1082; Leuchtenburg, "In the Shadow of FDR," 1185; Levi, "Committee of Vigilance," 230; Lieven, "Russia and the Origins of the First World War," 1362; Lloyd, "The State, France, and the Sixteenth Century," 130; Lythgoe, "Let 'Em Holler," 226; Maiorini, "Il Mouvement Republicain Populaire partito della IV Repubblica," 1086; Maizlish, "The Triumph of Sectionalism," 1158; Manor, "Political Change in an Indian State," 506; Manselli and Riedmann, editors, "Federico Barbarossa nel dibattito storiografico in Italia e in Germania," 114; Martin, "L'Idée de royauté à Rome," 413; Misiunas and Taagepera, "The Baltic States," 807; Morales Carrión *et al.*, "Puerto Rico," 889; Niven, "Martin Van Buren," 199; Nobles, "Divisions throughout the Whole," 1146; Olien, "Morpeh," 432; Pérez, "Cuba between Empires, 1878–1902," 1423; Persell, "The French Colonial Lobby, 1889–1938," 1086; Philip, "Oil and Politics in Latin America," 1187; Phillips, "The Sectarian Spirit," 433; Pole, "The Gift of Government," 735; Porter, "Britain, Europe, and the World, 1850–1982," 772; Potter, "The Liberty We Seek," 513; Radosh and Milton, "The Rosenberg File," 873; Rahe, *The Primacy of Politics in Classical Greece*, 265–93; Randers-Pehrson, "Barbarians and Romans," 415; Redžić, "Austromarksizam i jugoslavensko pitanje," 804; Redžić, "Jugoslavenska misao i socijalizam," 804; Ribuffo, "The Old Christian Right," 1185; Rioux, "La France de la IV^e République," vol. 2, "L'Expansion et l'impuissance, 1952–1958," 785; Romasco, "The Politics of Recovery," 868; Rybakov, "Kievskaja Rus' i russkie kniazhestva, XII–XIII vv.," 487; Seip, "The South Returns to Congress," 525; Shaw, "The Management of Scottish Society, 1707–1764," 776; Sirianni, "Workers Control and Socialist Democracy," 1122; Skowronek, "Building a New American State," 213; Smith, "The Church and Politics in Chile," 1193; "Islam and the Search for Social Order in Modern Egypt," 1128; Smith, Symcox, "Victor Amadeus II," 1355; Tent, "Mission on the Rhine," 154; Thornton, "The Kingdom of Kongo," 1130; Tjerneld, "Fråan borgarståndets storhetstid," 1095; Van Dijk, "Rebellion under the Banner of Islam," 190; Vann, "The Making of a State," 1347; Von Falkenstein, "Der Kampf der Tschechen um die historischen Rechte der böhmischen Krone im Spiegel der Presse, 1861–1879," 166; Vucinich, editor, "The First Serbian Uprising, 1804–1813," 486; Waterbury, "The Egypt of Nasser and Sadat," 1127; Weir, "Colonial South Carolina," 511; Weiss, "Farewell to the Party of Lincoln," 1404; Whelan, "Alexander III and the State Council," 169; Woodward, "Lloyd George and the Generals," 774 "Political Leadership in Jefferson's Virginia," by Jordan, 1152 "Political Parties in the West Bank under the Jordanian Regime, 1949–1967," by Cohen, 176 "Political Repression in 19th-Century Europe," by Goldstein, 1044
- POLITICAL SCIENCE: Apostolidès, "Le roi-machine," 451; Bauer, "Flüchtlinge und Flüchtlingspolitik in Bayern, 1945–1950," 154; Campbell, "Representative Democracy," 530; Conniff, editor, "Latin American Populism in Comparative Perspective," 564; Emmons, "The Formation of Political Parties and the First National Elections in Russia," 812; Foweraker, "The Struggle for Land," 1193; Glee, "Exile Politics during the Second World War," 127; Gossweiler, "Kapital, Reichswehr und NSDAP, 1919–1924," 148; Grüber, "Die Spitzenverbände der Wirtschaft und das erste Kabinett Brüning," 149; Jablon, "Crossroads of Decision," 1404; Katzburg, "Hungary and the Jews," 166; Kleppner, "Who Voted?" 214; Mavrogordatos, "Stillborn Republic," 162; Wald, "Crosses on the Ballot," 436 "Politics and Force Levels," by Ball, 554 "Politics, Finance, and the Church in the Reign of Edward II," by Buck, 1322 "The Politics of Aristocratic Empires," by Kautsky, 1043 "The Politics of Industrial Mobilization in Russia, 1914–17," by Siegelbaum, 1360 "The Politics of Locke's Philosophy," by Wood, 735 "The Politics of Recovery," by Romasco, 868 "Politische Polizei zwischen Demokratie und Diktatur," by Graf, 467 "Polityka wielkiej Brytanii wobec Niemiec w latach 1918–1925," by Kraszewski, 774 Pollins, Harold (R), 777 Pollins, Harold, "Economic History of the Jews in England," 1078 Pond, Arthur, 1327 Ponting, K. G., and D. T. Jenkins, "The British Wool Textile Industry, 1770–1914," 1074 Ponting, K. G., and N. B. Harte, editors, "Cloth and Clothing in Medieval Europe: Essays in Memory of Professor E. M. Carus-Wilson" (E), 1427 "Popular Opinion and Political Dissent in the Third Reich," by Kershaw, 793 "Populatia rurală a României între cele două războaie mondiale," by Șandru, 163 "Population Studies from Parish Registers," edited by Drake (E), 572 Porter, Bernard, "Britain, Europe, and the World, 1850–1982: Delusions of Grandeur," 772

- Porter, Vincent, and James Curran, editors, "British Cinema History" (E), 897
- "Il porto di Trapani nel Settecento," by Benigno, 1354
- Posnansky, Merrick (R), 822
- Post, Robert C. (R), 1174
- Poster, Mark (R), 736
- Postma, Folkert, "Viglius van Aytta als humanist en diplomaat, 1507–1549," 1343
- Potash, Robert A. (R), 239
- Potter, Janice, "The Liberty We Seek: Loyalist Ideology in Colonial New York and Massachusetts," 513
- Poussou, Jean-Pierre, "Bordeaux et le sud-ouest au XVIII^e siècle: Croissance économique et attraction urbaine," 1083
- "Le pouvoir et la plume," by Guidi *et al.* (E), 574
- "Pouvoir, ville et société en Europe, 1650–1750," edited by Livet and Vogler (E), 567
- Powell, Philip Wayne, *et al.*, "Essays on Frontiers in World History" (E), 894
- "Power and Protest in the Countryside," edited by Weller and Guggenheim (E), 243
- "Power and Society," by Hammack, 213
- "The Power of Power Politics," by Vasquez, 1046
- "Power, Strategy, and Security," edited by Knorr (E), 242
- "The Powers of Prophecy," by Lerner, 746
- "The Pragmatic Imagination," by Sass, 216
- Prawer, Joshua, 111
- "Precious Metals in the Later Medieval and Early Modern Worlds," edited by Richards (E), 896
- "The Prehistory of Germanic Europe," by Schutz, 1310
- "The Prehistory of the Balkans; and the Middle East and the Aegean World, Tenth to Eighth Centuries B.C.," vol. 3, pt. 1 of "The Cambridge Ancient History," edited by Boardman *et al.*, 1054
- "Prelude to Partition," by Page, 505
- "The Prelude to the Truman Doctrine," by Alexander, 479
- "Presbyterian Women in America," by Boyd and Brackenridge, 862
- "The Presidents," edited by Graff (E), 1431
- Presidents of the American Historical Association: A Statistical Analysis* (Research Note), by Pocock, 1016–24
- Press, Volker, editor, "Städtewesen und Merkantilismus in Mitteleuropa," 1349
- Price, Arnold H. (R), 146
- "Priests and Power," by Maier, 1372
- The Primacy of Politics in Classical Greece*, by Rahe, 265–93
- "Prime Ministers and Diplomats," by Edwards, 1141
- "Princes and Peasants," by Hopkins, 1049
- "Printing and Society in Early America," edited by Joyce *et al.* (E), 899
- Prior, Mary, "Fisher Row: Fishermen, Bargemen, and Canal Boatmen in Oxford, 1500–1900," 429
- Prior, Robin, "Churchill's *World Crisis* as History," 125
- "A Prison of Expectations," by Mintz, 431
- "Prisoners of Hope," by Hughes, 802
- "Private Academies of Tokugawa Japan," by Rubinger, 1376
- "Private Power for the Public Good," by Lagemann, 551
- "Prix et salaires à Florence au XIV^e siècle. 1280–1380," by de la Roncière, 418
- "Probability and Certainty in Seventeenth-Century England," by Shapiro, 762
- "Proceedings of the Battle Conference on Anglo-Norman Studies," vol. 4, "1981," edited by Brown (E), 245
- "Il processo d'unificazione nella realtà del paese, 1861–1887" (E), 574
- Prochaska, Alice, "History of the General Federation of Trade Unions, 1899–1980," 125
- Proctor, Raymond L., "Hitler's Luftwaffe in the Spanish Civil War," 1352
- "Professions and Professional Ideologies in America," edited by Geison (E), 1202
- "Profils Prussiens" by Haffner and Venohr (E), 898
- Prokof'eva, L. S., "Krest'ianskaia obshchina v Rossii vo vtoroi polovine XVIII–pervoi polovine XIV v. (Na materialakh votchinn Sheremetevykh)," 811
- "Propaganda and the German Cinema, 1933–1945," by Welch, 466
- "Property, Family, and the Legal Profession," vol. 2 of "Women and the Law," edited by Weisberg (E), 571
- PROSOPOGRAPHY: Cockshaw, "Le personnel de la chancellerie de Bourgogne-Flandre sous les ducs de Bourgogne de la maison de Valois, 1384–1477," 114; Syme, "Some Arval Brethren," 414
- "Protest und Repression im Vormärz," by Husung, 1100
- "Protestant Reformers in Elizabethan Oxford," by Dent, 1065
- PROTESTANTISM: Ash, "Protestantism and the American University," 222; Boyd and Brackenridge, "Presbyterian Women in America," 862; Collinson, "The Religion of Protestants," 761; Dent, "Protestant Reformers in Elizabethan Oxford," 1065; Hill *et al.*, "The World of the Muggletonians," 426; Hunt, "The Puritan Moment," 117; Jacob, "Henry Stubbe, Radical Protestantism, and the Early Enlightenment," 426; Jordan, "The Evangelical Alliance for the United States of America, 1847–1900," 861; Keller, "American Protestantism and United States Indian Policy, 1869–1882," 208; Lindberg, "The Third Reformation?" 1040; Nawyn, "American Protestantism's Response to Germany's Jews and Refugees, 1933–1941," 872; Reed, "The Missionary Mind and American East Asia Policy, 1911–1915," 537; Szasz, "The Divided Mind of Protestant America, 1880–1930," 861; Weeks, "Kentucky Presbyterians," 1384; Wilson, "The Benevolent Deity," 1385
- "Protestantism and the American University," by Ash, 222
- Provenzo, Eugene F., Jr., and H. Warren Button (C), 1222
- Prude, Jonathan, "The Coming of Industrial Order: Town and Factory Life in Rural Massachusetts, 1810–1860," 1157
- "Psychic Conflict in Spanish America," by Goldwert (E), 254
- PSYCHOBIOGRAPHY AND PSYCHOHISTORY: Carlton, "Charles I," 1066; Hoffer, "Revolution and Regeneration," 1389; Hughes, "Emotion and High Politics," 407; Loewenberg, "Decoding the Past," 94; Marvick, "The Young Richelieu," 1333; Wurgaft, "The Imperial Imagination," 831
- PSYCHOLOGY: Klein, "Jewish Origins of the Psychoanalytic Movement," 408; Pugh, "Sons of Liberty," 1382; Sokal, *The Gestalt Psychologists in Behaviorist America*, 1240–63
- "Public Life in Renaissance Florence," by Trexler, 475
- "Public Property and Private Power," by Hartog, 1149
- "Puerto Rican Politics in Urban America," edited by Jennings and Rivera (E), 1431
- "Puerto Rico," by Morales Carrión *et al.*, 889

- Pugh, David G., "Sons of Liberty: The Masculine Mind in Nineteenth-Century America," 1382
- Pugh, M. D. (R), 436
- Pullan, Brian, "The Jews of Europe and the Inquisition of Venice, 1550–1670," 1109
- "Puolueen aseet," by Salokangas, 790
- "The Puritan Moment," by Hunt, 117
- "The Pursuit of Happiness," by Lewis, 1148
- "The Pursuit of Isolationism in the United States Senate from Versailles to Pearl Harbor," by Guinsburg, 882
- "The Pursuit of Urban History," edited by Fraser and Sutcliffe (E), 368
- Pusey, James Reeve, "China and Charles Darwin," 1135
- Pushkareva, I. M., "Fevral'skaia burzhuazno-demokraticeskaiia revoliutsiia 1917 g. v Rossii," 173
- Puskás, Julianna, "From Hungary to the United States, 1880–1914," 165
- Pyron, Darden Asbury, editor, "Recasting: *Gone with the Wind* in American Culture" (E), 900
- QUANTITATIVE HISTORY: Levy, "War in the Modern Great Power System, 1495–1975," 1046; Pocock, *Presidents of the American Historical Association: A Statistical Analysis* (Research Note), 1016–24
- "Queens, Concubines, and Dowagers," by Stafford, 106
- "Quetzalcoatl and the Irony of Empire," by Carrasco, 1417
- "Qui venit contra iura," by Boari, 1105
- "Quiet Revolution," by Shafer, 1414
- Quigley, Carroll, "Weapons Systems and Political Stability: A History," 98
- Quiring-Zoeche, Rosemarie, "Isfahan im 15. und 16. Jahrhundert: Ein Beitrag zur persischen Stadtgeschichte," 499
- Raban, Sandra, "Mortmain Legislation and the English Church, 1279–1500," 416
- "Rabbi Moses Nahmanides (Ramban)," edited by Twersky (E), 1196
- Rabinbach, Anson, "The Crisis of Austrian Socialism: From Red Vienna to Civil War, 1927–1934," 160
- RACE AND RACE RELATIONS: Barnes, "Journey from Jim Crow," 1411; Cox, "Blacks in Topeka, Kansas, 1865–1915," 859; De León, "They Called Them Greasers," 857; Eagles, "Jonathan Daniels and Race Relations," 224; Ellsworth, "Death in a Promised Land," 859; Reid, "A Nest of Hornets," 1379; Rothschild, "A Case of Black and White," 230; Snowden, "Before Color Prejudice," 103
- "Racism and Colonialism," edited by Ross (E), 243
- Rackham, Oliver, "Ancient Woodland: Its History, Vegetation, and Uses in England," 755
- "Radical General," by Spiers, 1071
- RADICALISM: Himka, "Socialism in Galicia," 1114; Salmond, "A Southern Rebel," 224; Wells, "Insurrection," 766
- "Radicalism and Freethought in Nineteenth-Century Britain," by Wiener, 432
- Radosh, Ronald, and Joyce Milton, "The Rosenberg File: A Search for the Truth," 873
- Raeff, Marc, "The Well-Ordered Police State: Social and Institutional Change through Law in the Germanies and Russia, 1600–1800," 404
- Raem, Heinz-Albert, "Katholischer Gesellenverein und deutsche Kolpingsfamilie in der Ära des Nationalsozialismus," 145
- Ragon, Michel, "The Space of Death: A Study of Funerary Architecture, Decoration, and Urbanism," 1307
- Ragosta, Rosalba, editor, "Le genti del mare Mediterraneo" (E), 248
- Rahe, Paul A. (R), 104
- Rahe, Paul A., *The Primacy of Politics in Classical Greece*, 265–93
- Raichle, Donald R., "New Jersey's Union College: A History, 1933–1983," 1408
- "Rail, finance, et politique," by Kurgan-van Hentenryk, 787
- Ramusack, Barbara N. (R), 503
- Randall, Laura (R), 1187
- Randers-Pehrson, Justine Davis, "Barbarians and Romans: The Birth Struggle of Europe, A.D. 400–700," 415
- "Randolph-Macon College," by Scanlon, 881
- Ranger, Terence, and Eric Hobsbawm, editors, "The Invention of Tradition" (E), 241
- Ránki, György, "Economy and Foreign Policy: The Struggle of the Great Powers for Hegemony in the Danube Valley, 1919–1939," 1104
- Ransel, David L. (R), 1097
- "Ranulf of Chester," by Alexander, 1321
- Ravitch, Diane, and Ronald K. Goodenow, editors, "Schools in Cities: Consensus and Conflict in American Educational History," 1176
- Ray, Rajat K., "Industrialization in India: Growth and Conflict in the Private Corporate Sector, 1914–47," 507
- Raymond, Ellsworth (R), 1362
- Read, Christopher (R), 169
- Reagan, Alice E., "H. I. Kimball, Entrepreneur," 526
- "Realistic Utopias," by Eliav-Feldon, 91
- Rearick, Charles (R), 782
- "Reason of State and Statecraft in Spanish Political Thought, 1595–1640," by Fernández-Santamaría, 457
- "Rebel," by Siepel, 852
- "Rebel for Rights," by Moynihan, 860
- Rebel, Hermann, "Peasant Classes: The Bureaucratization of Property and Family Relations under Early Habsburg Absolutism, 1511–1636," 159
- "Rebellion and Democracy in Meiji Japan," by Bowen, 187
- "Rebellion under the Banner of Islam," by Van Dijk, 190
- "The Rebirth of the Missouri Pacific, 1956–1983," by Miner, 1410
- "Recasting," edited by Pyron (E), 900
- Rechtien, John G. (R), 757
- "Red and Hot," by Starr, 174
- Redclift, Michael, and David Goodman, "From Peasant to Proletarian: Capitalist Development and Agrarian Transitions," 1300
- Redžić, Enver, "Austromarksizam i jugoslavensko pitanje," 804
- Redžić, Enver, "Jugoslovenska misao i socijalizam," 804
- Reed, Germaine (R), 881
- Reed, James, "The Missionary Mind and American East Asia Policy, 1911–1915," 537
- Reeves, A. Compton (R), 1322
- Reeves, Thomas C. (R), 1414
- "Reflections on de Gaulle," by Morrissey, 1340
- "Reform and Reaction in the Platine Provinces, 1810–1852," by Bushnell, 1420
- REFORMATION: Brooks, editor, "Seven-Headed

- Luther," 1346; Edwards, "Luther's Last Battles," 463; Schüler, "Die Klostersäkularisation in Kent, 1535–1558," 759
- "Réformisme et révisionnisme dans les socialismes allemand, autrichien et français," edited by Dreyfus (E), 1428
- "Regierung, Bürokratie und Parlament in Preussen und Deutschland von 1848 bis zur Gegenwart," edited by Ritter (E), 897
- REGIONAL AND PROVINCIAL HISTORY: Beck, "Joseph Howe," vol. 1, "Conservative Reformer, 1804–1848," 235; Boardman *et al.*, editors, "The Cambridge Ancient History," vol. 3, pt. 1, "The Prehistory of the Balkans; and the Middle East and the Aegean World, Tenth to Eighth Centuries B.C.," 1054; Bonner, "Kings, Commoners, and Concessionaires," 825; Bowersock, "Roman Arabia," 1060; Boyer, "America's Virgin Islands," 1423; Brantley, "The Giraama and Colonial Resistance in Kenya, 1800–1920," 824; Bushnell, "Reform and Reaction in the Platine Provinces, 1810–1852," 1420; Carreira, "The People of the Cape Verde Islands," 179; Chandler, "A History of Cambodia," 834; Chang, "Art, Myth, and Ritual," 1373; Cronon, "Changes in the Land," 839; Fairbank, editor, "The Cambridge History of China," vol. 12, pt. 1, "Republican China, 1912–1949," 1133; Fine, "The Ancient Greeks," 1312; Getzler, "Kronstadt, 1917–1921," 492; Goodman, "State and Society in Roman Galilee, A.D. 132–212," 1315; Grantham, "Southern Progressivism," 1403; Gregory, "Regional Transformation and Industrial Revolution," 1074; Gunawardana, "Robe and Plough," 188; Hahn, "The Roots of Southern Populism," 854; Hahn, "Wirtschaftliche Integration im 19. Jahrhundert," 146; Hearnden, "Independence and Empire," 211; Helms, "The Cohesion of Saudi Arabia," 819; Hobson, "Tell about the South," 1381; Home, "City of Blood Revisited," 180; Ishaq Khan, "Perspectives on Kashmir," 1378; Jelavich, "History of the Balkans," vol. 1, "Eighteenth and Nineteenth Centuries"; vol. 2, "Twentieth Century," 1116; Keeding, "Das Zeitalter der Aufklärung in der Provinz Quito," 1419; Kruman, "Parties and Politics in North Carolina, 1836–1865," 849; Lewis, "Main Currents in Caribbean Thought," 887; Mallon, "The Defense of Community in Peru's Central Highlands," 890; McKitterick, "The Frankish Kingdoms under the Carolingians, 751–987," 1317; Moch, "Paths to the City," 782; Nashat, "The Origins of Modern Reform in Iran, 1870–80," 1367; Ocko, "Bureaucratic Reform in Provincial China," 182; Robson, "A History of Tasmania," vol. 1, "Van Diemen's Land from the Earliest Times to 1855," 1143; Scharer, "Die angelsächsische Königsurkunde im 7. und 8. Jahrhundert," 1063; Schutz, "The Prehistory of Germanic Europe," 1310; Sprunger, "Dutch Puritanism," 788; Taylor, "The Birth of Vietnam," 834; Tent, "Mission on the Rhine," 154; Toby, "State and Diplomacy in Early Modern Japan," 1375; Turner, "The Ninth State," 847; Varg, "New England and Foreign Relations, 1789–1850," 197; Waters, "Japan's Local Pragmatists," 827; Yarshater, editor, "The Cambridge History of Iran," vol. 3, "The Seleucid, Parthian, and Sasanian Periods," 1055; Yudelman, "The Emergence of Modern South Africa," 181
- "Regional Transformation and Industrial Revolution," by Gregory, 1074
- "Regulating Society," by Mizruchi, 410
- "The Regulation and Reform of the American Banking System, 1900–1929," by White, 217
- Rehbock, Philip F., "The Philosophical Naturalists: Themes in Early Nineteenth-Century British Biology," 1072
- "Reichsstädte, freie Städte und Königtum, 1389–1450," by Heinig, 1345
- "Reichsständische Repräsentationsformen im 16. Jahrhundert," by Neuhaus, 463
- Reid, Donald M. (R), 498
- Reid, Gordon, "A Nest of Hornets: The Massacre of the Fraser Family at Horner Bank Station, Central Queensland, 1857, and Related Events," 1379
- Reinfeld, Barbara (R), 482
- Reingold, Nathan (R), 534
- Reinhard, David W., "The Republican Right since 1945," 1413
- Reisler, Mark (R), 857
- Reissner, Johannes, "Ideologie und Politik der Muslimbrüder Syriens: Von den Wahlen 1947 bis zum Verbot unter Adib as-Sisakli 1952," 819
- RELIGION AND THEOLOGY: Bentley, "Humanists and Holy Writ," 1039; Boyle, "Rhetoric and Reform," 1098; Brooks, editor, "Seven-Headed Luther," 1346; Buck, "Politics, Finance, and the Church in the Reign of Edward II," 1322; Buschkühl, "Great Britain and the Holy See, 1746–1870," 1068; Chadwick, "Hensley Henson," 1080; Chernaik, "The Poet's Time," 425; D'Amico, "Renaissance Humanism in Papal Rome," 1106; de Montclos, "Les chrétiens face au nazisme et au stalinisme," 1306; Essig, "The Bonds of Wickedness," 196; Finlayson, "Historians, Puritanism, and the English Revolution," 1066; Frantzen, "The Literature of Penance in Anglo-Saxon England," 417; Friedman, "The Most Ancient Testimony," 1294; Hayman, "Catholicism in Rhode Island and the Diocese of Providence, 1780–1886," 1151; Hunt, "Holy Land Pilgrimage in the Later Roman Empire, A.D. 312–460," 106; Jones, "Faith by Statute," 1325; Jordan, "The Evangelical Alliance for the United States of America, 1847–1900," 861; Kselman, "Miracles and Prophecies in Nineteenth-Century France," 782; Lindberg, "The Third Reformation?" 1040; Loetscher, "Facing the Enlightenment and Pietism," 202; Manuel, "The Changing of the Gods," 1047; McIntire, "England against the Papacy, 1858–1861," 1068; McLoughlin, "Cherokees and Missionaries, 1789–1839," 1390; Mikalson, "Athenian Popular Religion," 1057; Olson, "Science Deified and Science Defied," 99; Phillips, "Jedidiah Morse and New England Congregationalism," 515; Phillips, "The Sectarian Spirit," 433; Raban, "Mortmain Legislation and the English Church, 1279–1500," 416; Ribuffo, "The Old Christian Right," 1185; Rodes, "Lay Authority and Reformation in the English Church," 760; Scheibelreiter, "Der Bischof in merowingischer Zeit," 1317; Schumacher, "Revolutionary Clergy," 1140; Sonne, "Saints on the Seas," 851; Szasz, "The Divided Mind of Protestant America, 1880–1930," 861; Thrower, "Marxist-Leninist Scientific Atheism and the Study of Religion and Atheism in the USSR," 817; Wilken, "John Chrysostom and the Jews," 1059; Wooden, "John Foxe," 758
- "The Religion of Protestants," by Collinson, 761
- "The Reluctant Patron," by Larson, 552
- "Remember Kirkland Lake," by MacDowell, 557
- RENAISSANCE: Benson *et al.*, editors, "Renaissance and Renewal in the Twelfth Century," 109; Bentley, "Humanists and Holy Writ," 1039; Eliav-Feldon,

- "Realistic Utopias," 91; Warnicke, "Women of the English Renaissance and Reformation," 421
- "The Renaissance and Reformation in Scotland," edited by Cowan and Shaw (E), 572
- "Renaissance and Renewal in the Twelfth Century," edited by Benson *et al.*, 109
- "Renaissance Humanism in Papal Rome," by D'Amico, 1106
- "Renato Beluche," by De Grummond, 887
- Renaud, Bertrand (R), 502
- Renger, Christian, "Die Gründung und Einrichtung der Universität Bonn und die Berufungspolitik des Kultusministers Altenstein," 143
- Renzi, William A. (R), 801
- "Representative Democracy," by Campbell, 530
- "Republican China, 1912–1949," vol. 12, pt. 1 of "The Cambridge History of China," edited by Fairbank, 1133
- "The Republican Right since 1945," by Reinhard, 1413
- "Die Republik Texas, Bremen und das Hildesheimische," by Struve, 1155
- "The Reshaping of Plantation Society," by Wayne, 521
- "The *Resolution* Journal of Johann Reinhold Forster, 1772–1775," edited by Hoare, 120
- "Rethinking Intellectual History," by LaCapra, 1048
- Reuther, Walter, 227
- "Revivalism, Social Conscience, and Community in the Burned-Over District," by Altschuler and Saltzgeber, 204
- REVOLUTION: Beevor, "The Spanish Civil War," 1090; Bogin, "Abraham Clark and the Quest for Equality in the Revolutionary Era, 1774–1794," 194; Church, "Europe in 1830," 406; Dostian *et al.*, editors, "Pervoe serbskoe vosstanie 1804–1813 gg. i Rossiia," vol. 2, "1808–1813," 1359; Dozier, "For King, Constitution, and Country," 1328; Elliott, "Partners in Revolution," 129; Ezergailis, "The Latvian Impact on the Bolshevik Revolution," 1121; Hoffman and Albert, editors, "Arms and Independence," 1387; Kürkcüoğlu, "Osmanlı Devleti'ne Karsi Arap Bağimsizlik Hareketi, 1908–1918," 176; Mints, "God 1918i," 1360; Nafziger, "The Economics of Political Instability," 180; Naimark, "Terrorists and Social Democrats," 489; Pencak, "America's Burke," 194; Pushkareva, "Fevral'skaia burzhuazno-demokraticheskaiia revoliutsiia 1917 g. v Rossiia," 173; Rousset, "A Critical History of the USSR," vol. 1, "The Legacy of the Bolshevik Revolution," 495; Sutherland, "The Chouans," 780
- "Revolution and Intervention," by Carley, 1337
- "Revolution and Red Tape," by Church, 452
- "Revolution and Regeneration," by Hoffer, 1389
- "Revolution in Time," by Landes, 1052
- "Revolutionary Clergy," by Schumacher, 1140
- "Revolutionary Socialist Development in the Third World," edited by White *et al.* (E), 242
- "Revolutionary Women," by Mullaney, 1048
- Reynolds, Clark G. (R), 1406
- Reynolds, Clark W., and Carlos Tello, editors, "U.S.–Mexico Relations: Economic and Social Aspects" (E), 901
- Reynolds, Terry S., "Stronger than a Hundred Men: A History of the Vertical Water Wheel," 101
- Rhee, T. C. (R), 1377
- "Rhetoric and Reform," by Boyle, 1098
- "The Rhetoric of Conservatism," by Bruce, 518
- Ribuffo, Leo P. (R), 1406
- Ribuffo, Leo P., "The Old Christian Right: The Protestant Far Right from the Great Depression to the Cold War," 1185
- "Rice and Slaves," by Littlefield, 195
- Rice, Kym S., "Early American Taverns: For the Entertainment of Friends and Strangers," 1145
- Richard, Jean, "Saint Louis: Roi d'une France féodale, soutien de la Terre Sainte," 751
- Richards, Alan, and Philip L. Martin, editors, "Migration, Mechanization, and Agricultural Labor Markets in Egypt" (E), 1430
- Richards, J. F., editor, "Precious Metals in the Later Medieval and Early Modern Worlds" (E), 896
- Richardson, Bonham C., "Caribbean Migrants: Environment and Human Survival on St. Kitts and Nevis," 562
- Richardson, Edgar P., *et al.*, "Charles Willson Peale and His World," 847
- Richardson, Elmo, "David T. Mason, Forestry Advocate: His Role in the Application of Sustained Yield Management to Private and Public Forest Lands," 1182
- Richardson, James F. (R), 1401
- Richardson, Peter, and Shula Marks, editors, "International Labour Migration: Historical Perspectives" (E), 1425
- Richardson, R. Dan (R), 1352
- Richelieu, Armand Jean du Plessis, duc de (Cardinal), 1333
- Ricklefs, M. C. (R), 189
- Rieber, Alfred J. (R), 169
- Riedmann, Josef and Raoul Manselli, editors, "Federico Barbarossa nel dibattito storiografico in Italia e in Germania," 114
- Riemenschneider, Rainer, and Karl-Ernst Jeismann, editors, "Polen und Deutschland im Zeitalter der Aufklärung: Reformen im Bereich des politischen Lebens, der Verfassung und der Bildung" (E), 247
- Riess, Steven A. (R), 877
- Rigaudière, Albert, "Saint-Flour ville d'Auvergne au bas Moyen Age: Étude d'histoire administrative et financière," 753
- Riley, Glenda (R), 548
- Riley, James D. (R), 1419
- Rilling, John R. (R), 424
- "Riots and Community Politics in England and Wales, 1790–1810," by Bohstedt, 429
- Rioux, Jean-Pierre, "La France de la IV^e République," vol. 2, "L'Expansion et l'impuissance, 1952–1958," 785
- "The Rise and Decline of the American Cut Nail Industry," by Loveday, 1399
- "The Rise and Fall of Childhood," by Sommerville, 95
- "The Rise of the Great Powers, 1648–1815," by McKay and Scott, 405
- "The Rise of the Midwestern Meat Packing Industry," by Walsh, 529
- "Risks at Sea," by Spooner, 1092
- Ritchie, Robert C. (R), 1387
- Ritter, Gerhard A., editor, "Regierung, Bürokratie und Parlament in Preussen und Deutschland von 1848 bis zur Gegenwart" (E), 897
- Rivera, Monte, and James Jennings, editors, "Puerto Rican Politics in Urban America" (E), 1431
- Rivlin, Helen Anne B. (R), 1128
- "The Road to Berlin," by Erickson, 1122
- Robbins, William G. (R), 527
- "Robe and Plough," by Gunawardana, 188
- "Robert Hunter, 1666–1734," by Lustig, 1387
- Roberts, Warren (R), 1335
- Robertson, C. J. A., "The Origins of the Scottish Railway System, 1722–1844," 777

- Robertson, Claire C., and Martin A. Klein, editors, "Women and Slavery in Africa," 1370
- Robins-Mowry, Dorothy, "The Hidden Sun: Women of Modern Japan," 501
- Robinson, Jackie, 877
- Robson, Lloyd, "A History of Tasmania," vol. 1, "Van Diemen's Land from the Earliest Times to 1855," 1143
- "La Rochelle and the Atlantic Economy during the Eighteenth Century," by Clark, 779
- Rodes, Robert E., Jr., "Lay Authority and Reformation in the English Church: Edward I to the Civil War," 760
- Rodman, Margaret, and Matthew Cooper, editors, "The Pacification of Melanesia" (E), 1201
- Roebuck, Janet (R), 434
- Roebuck, Peter, editor, "Macartney of Lisanoure, 1737-1806: Essays in Biography" (E), 897
- Roeder, George H., Jr. (R), 1177
- Roederer, Pierre-Louis, 452
- Roff, William R. (R), 190
- Rogger, Hans (R), 491
- "Le roi-machine," by Apostolidès, 451
- Roider, Karl A., Jr. (R), 1111
- "Roman Arabia," by Bowersock, 1060
- "Roman Gaul," by Drinkwater, 1058
- "The Romance of Commerce and Culture," by Allen, 1409
- Romano, Sergio, "Giuseppe Volpi et l'Italie moderne: Finance, industrie et état de l'ère giolittienne à la Deuxième Guerre mondiale," 800
- Romasco, Albert U., "The Politics of Recovery: Roosevelt's New Deal," 868
- "Rome's North West Frontier," by Hanson and Maxwell, 743
- "Römische Geschichte und deutsche Geschichtswissenschaft," by Christ, 105
- Romo, Ricardo, "East Los Angeles: History of a Barrio," 544
- Rooke, Patricia T., and R. L. Schnell, "Discarding the Asylum: From Child Rescue to the Welfare State in English-Canada (1800-1950)," 1415
- Roosen, William (R), 451
- "Roosevelt and the Isolationists, 1932-45," by Cole, 538
- Roosevelt, Franklin Delano, 538; 867
- Roosevelt, Theodore, 1170
- Root, John D. (R), 444
- "Roots and Realities among Eastern and Central Europeans," edited by Kovacs (E), 250
- "The Roots of Southern Populism," by Hahn, 854
- Rösch, Gerhard, "Venedig und das Reich: Handels- und verkehrspolitische Beziehungen in der deutschen Kaiserzeit," 115
- Rosen, Andrew (R), 97
- "The Rosenberg File," by Radosh and Milton, 873
- Rosenberg, Julius and Ethel, 873
- Rosenthal, Franz (R), 1125
- Rosenthal, Joel T. (R), 1320
- Rosenwein, Barbara H. (R), 752
- Rosof, Patricia J. F., *et al.*, editors, "Ethnic and Immigration Groups: The United States, Canada, and England" (E), 241
- Rosof, Patricia J. F., *et al.*, editors, "The Middle East and North Africa: Medieval and Modern History" (E), 243
- Rosof, Patricia J. F., *et al.*, editors, "The Military and Society: Reviews of Recent Research" (E), 242
- Ross, Dorothy, *Historical Consciousness in Nineteenth-Century America*, 909-28
- Ross, Robert, editor, "Racism and Colonialism: Essays on Ideology and Social Structure" (E), 243
- Ross, Sul, 1162
- Rossabi, Morris, editor, "China among Equals: The Middle Kingdom and Its Neighbors, 10th-14th Centuries" (E), 577
- Rosbach, Jeffery, "Ambivalent Conspirators: John Brown, the Secret Six, and a Theory of Slave Violence," 520
- Rossi, Elena Aga, editor, "Il Piano Marshall e l'Europa" (E), 1199
- Rotella, Elyce J., "From Home to Office: U.S. Women at Work, 1870-1930," 1165
- Roth, Guenther (R), 1350
- Roth, Norman (R), 420
- Rothberg, Morey D., "To Set a Standard of Workmanship and Compel Men to Conform to it": John Franklin Jameson as Editor of the American Historical Review, 957-75
- Rothman, David J., "Conscience and Convenience: The Asylum and Its Alternatives in Progressive America," 227
- Rothschild, Mary Aickin, "A Case of Black and White: Northern Volunteers and the Southern Freedom Summers, 1964-1965," 230
- Rousseau, Jean Jacques, 1084
- Rousset, David, "A Critical History of the USSR," vol. 1, "The Legacy of the Bolshevik Revolution," 495
- Rowan, Steven (R), 1345
- Rowen, Herbert H. (R), 405
- Rowse, A. L., "Eminent Elizabethans," 424
- "The Royal Dockyards during the Revolutionary and Napoleonic Wars," by Morriss, 1328
- Royle, Edward (R), 116
- Rubanowice, Robert J., "Crisis in Consciousness: The Thought of Ernst Troeltsch," 791
- Rubinger, Richard, "Private Academies of Tokugawa Japan," 1376
- Rubinstein, W. D. (R), 443
- Ruderman, David (R), 1294
- Rudwick, Elliott, and August Meier, *J. Franklin Jameson, Carter G. Woodson, and the Foundations of Black Historiography* (Research Note), 1005-15
- Ruff, Julius R. (R), 1083
- "Rufus Choate," by Matthews, 1155
- Rufus, William, 1319
- Ruggiero, Guido (R), 1105
- "Rulers, Townsmen, and Bazaars," by Bayly, 504
- Rumpler, Helmut, and Heinrich Lutz, editors, "Österreich und die deutsche Frage im 19. und 20. Jahrhundert: Probleme der politisch-staatlichen und soziokulturellen Differenzierung im deutschen Mitteleuropa" (E), 898
- Russell, William Howard, 767
- "Russia and the Origins of the First World War," by Lieven, 1362
- "La Russia rivoluzionaria nella politica italiana," by Petracchi, 477
- "The Russian Baltic Provinces between the 1905/1917 Revolutions," edited by Ezergailis and Von Pistohlkors (E), 251
- "Russian Shadows on the British Northwest Coast of North America, 1810-1890," by Barratt, 1044
- Russo, Carla, and Giuseppe Galasso, editors, "Per la storia sociale e religiosa del Mezzogiorno d'Italia" (E), 248
- Rutherford, Paul, "A Victorian Authority: The Daily Press in Late Nineteenth-Century Canada," 237
- Rutyna, Richard A., and Peter C. Stewart, editors, "Virginia in the American Revolution: A Collection of Essays" (E), 253
- Ryan, Dennis P., "Beyond the Ballot Box: A Social History of the Boston Irish, 1845-1917," 523
- Ryan, Mary P., "The Empire of the Mother:

- American Writing about Domesticity, 1830–1860," 205
- Rybakov, B. A., "Kievskaja Rus'i russkie kniazhestva, XII–XIII vv.," 487
- Rystad, Göran, editor, "Europe and Scandinavia: Aspects of the Process of Integration in the Seventeenth Century" (E), 1196
- Saab, Ann Pottinger (R), 453
- Saad, Elias N., "Social History of Timbuktu: The Role of Muslim Scholars and Notables, 1400–1900," 1371
- Sachs, Carolyn E., "The Invisible Farmers: Women in Agricultural Production," 548
- Sack, James J. (R), 766
- "Sacred Sands," by Engel, 535
- "Sailor-Diplomat," by Long, 849
- St. Clair, Kenneth E. (R), 1389
- "Saint Louis," by Richard, 751
- "Saint Louis et son siècle," by Sivéry, 1324
- "St. Petersburg och Finland," by Engman, 1097
- "St. Thomas Cantilupe, Bishop of Hereford," edited by Jancey (E), 245
- "Saint-Flour ville d'Auvergne au bas Moyen Age," by Rigaudière, 753
- "Saints and Their Cults," edited by Wilson (E), 1426
- "Saints on the Seas," by Sonne, 851
- Saller, Richard P. (R), 740
- Salmon, E. T. (R), 413
- Salmond, John A., "A Southern Rebel: The Life and Times of Aubrey Willis Williams, 1890–1965," 224
- Salokangas, Raimo, "Puolueen aseet: Maalaisliiton sanomalehdistön synty ja sen asema puolueessa ja sanomalehtimarkkinoilla, 1906–1916," 790
- Saltzgeber, Jan M., and Glenn C. Altschuler, "Revivalism, Social Conscience, and Community in the Burned-Over District: The Trial of Rhoda Bement," 204
- Salvatore, Nick (R), 230
- Salvucci, Richard J. (R), 1189
- Sánchez Korrol, Virginia E., "From Colonia to Community: The History of Puerto Ricans in New York City, 1917–1948," 878
- Sandberg, Lars G. (R), 462
- Sandeén, Ernest R., editor, "The Bible and Social Reform" (E), 252
- Sanders, M. L., and Philip M. Taylor, "British Propaganda during the First World War, 1914–18," 1081
- Sandford, Gregory W., "From Hitler to Ulbricht: The Communist Reconstruction of East Germany, 1945–46," 473
- Sandhu, Kernial Singh, and Paul Wheatley, editors, "Melaka: The Transformation of a Malay Capital, c. 1400–1980," 835
- Şandru, D., "Populația rurală a României între cele două războaie mondiale," 163
- "The Santa Fe and Taos Colonies," by Gibson, 536
- "Sardis from Prehistoric to Roman Times," by Hanfmann *et al.*, 739
- Sarpi, Paolo, 1109
- Sarti, Roland (R), 800
- Sass, Steven A., "The Pragmatic Imagination: A History of the Wharton School, 1881–1981," 216
- Sather, Leland B. (R), 1093; 1344
- Saunders, Kay, editor, "Indentured Labour in the British Empire, 1834–1920" (E), 1426
- "Savoring the Past," by Wheaton, 450
- "Sawdust Empire," by Maxwell and Baker, 527
- Sawyer, P. H., "Kings and Vikings: Scandinavia and Europe, A.D. 700–1100," 745
- Sayre, Stephen, 1147
- Scalapino, Robert A., and Jun-Yop Kim, editors, "North Korea Today: Strategic and Domestic Issues" (E), 577
- Scalapino, Robert A., and Jusuf Wanandi, editors, "Economic, Political, and Security Issues in Southeast Asia in the 1980s" (E), 577
- "Scandinavia during the Second World War," edited by Nissen, 1092
- Scanlon, James Edward, "Randolph-Macon College: A Southern History, 1825–1967," 881
- Scarborough, Ruth, "Belle Boyd: Siren of the South," 853
- Schaeper, Thomas J., "The French Council of Commerce, 1700–1715: A Study of Mercantilism after Colbert," 131
- Schaller, Michael (R), 232
- Scharer, Anton, "Die angelsächsische Königsurkunde im 7. und 8. Jahrhundert," 1063
- Scharf, Claus, and Hans-Jürgen Schröder, editors, "Die Deutschlandpolitik Frankreichs und die französische Zone, 1945–1949" (E), 899
- Scheibelreiter, Georg, "Der Bischof in merowingischer Zeit," 1317
- Scheibler, Ingeborg, "Griechische Töpferkunst: Herstellung, Handel und Gebrauch der Antiken Tongefäße," 1311
- Schelbert, Leo (R), 1155
- Schieder, Wolfgang, editor, "Liberalismus in der Gesellschaft des deutschen Vormärz," 1348
- Schlatter, Richard (R), 91; 735
- Schlebecker, John T. (R), 1161
- Schmitt, Carl, 152
- Schmitt, Charles B., "Aristotle and the Renaissance," 755
- Schmitt, Charles B., "John Case and Aristotelianism in Renaissance England," 757
- Schmitt, Hans A. (R), 785
- Schmitt, Jean-Claude, "The Holy Greyhound: Guinefort, Healer of Children since the Thirteenth Century," 1063
- Schmukler, Nathan, and Edward Marcus, editors, "Inflation through the Ages: Economic, Social, Psychological, and Historical Aspects" (E), 894
- Schneer, Jonathan, "Ben Tillet: Portrait of a Labour Leader," 445
- Schneider, Jürgen, and Wolf Dietrich Fischer, editors, "Das Heilige Land im Mittelalter: Begegnungsraum zwischen Orient und Okzident" (E), 244
- Schneider, William H. (R), 133
- Schnell, R. L., and Patricia T. Rooke, "Discarding the Asylum: From Child Rescue to the Welfare State in English-Canada (1800–1950)," 1415
- Schober, Gottlieb, 842
- Schoenl, William J., "The Intellectual Crisis in English Catholicism: Liberal Catholics, Modernists, and the Vatican in the Late Nineteenth and Early Twentieth Centuries," 441
- Schofer, Lawrence (R), 1111
- "Scholars and Dollars," by Axelrod, 237
- "Schools in Cities," edited by Goodenow and Ravitch, 1176
- Schöttler, Peter, "Die Entstehung der 'Bourses du travail': Sozialpolitik und französischer Syndikalismus am Ende des 19. Jahrhunderts," 456
- Schrader, Robert Fay, "The Indian Arts and Crafts Board: An Aspect of New Deal Indian Policy," 871
- Schrank, Gilbert (R), 448
- Schrecker, Ellen (R), 873
- Schrepfer, Susan R. (R), 1182

- Schrepfer, Susan R., "The Fight to Save the Redwoods: A History of Environmental Reform, 1917-1978," 534
- Schröder, Hans-Jürgen, and Claus Scharf, editors, "Die Deutschlandpolitik Frankreichs und die französische Zone, 1945-1949" (E), 899
- Schuker, Stephen A. (C), 1221
- Schüler, Sibylle, "Die Klostersäkularisation in Kent, 1535-1558," 759
- Schultz, Joseph P. (R), 1178
- Schulz, Günther (R), 1351
- Schulze, Hagen, and Hans-Dietrich Looock, editors, "Parlamentarismus und Demokratie im Europa des 19. Jahrhunderts" (E), 242
- Schulze, Leonard, and Walter Wetzles, editors, "Literature and History" (E), 572
- Schulzinger, Robert D. (R), 1404
- Schumacher, John N., "Revolutionary Clergy: The Filipino Clergy and the Nationalist Movement, 1850-1903," 1140
- Schumacher, Leonhard, "Servus Index: Sklavenverhör und Sklavenanzeige im republikanischen und kaiserzeitlichen Rom," 1314
- Schumann, Reinhold (R), 114; 1323
- Schutz, Herbert, "The Prehistory of Germanic Europe," 1310
- Schwarzlose, Richard A. (R), 876
- Schweider, Dorothy, "Black Diamonds: Life and Work in Iowa's Coal Mining Communities, 1895-1925," 229
- Schweitzer, Frederick M. (R), 440
- "Science and Society in Restoration England," by Hunter, 763
- "Science Deified and Science Defied," by Olson, 99
- Scott, H. M., and Derek McKay, "The Rise of the Great Powers, 1648-1815," 405
- Scott, Rebecca J. (R), 888
- Scott, Samuel F. (R), 452
- "Scottish Culture and Scottish Education, 1800-1980," edited by Humes and Paterson (E), 246
- Scull, Andrew, and Stanley Cohen, editors, "Social Control and the State," 1305
- Scullard, H. H., "Festivals and Ceremonies of the Roman Republic," 739
- Sealand, Judith, "As Minority Becomes Majority: Federal Reaction to the Phenomenon of Women in the Work Force, 1920-1963," 547
- Seaver, James E. (R), 1059
- Seaver, Paul S. (R), 1065
- "The Second British Empire," by Halstead, 1076
- "Secondary School Reform in Imperial Germany," by Albisetti, 144
- "The Sectarian Spirit," by Phillips, 433
- "The Secular Ark," by Browne, 100
- "The Secularization of the Soul," by Cerullo, 444
- Sedlak, Michael W., and Harold F. Williamson, "The Evolution of Management Education: A History of the Northwestern University J. L. Kellogg Graduate School of Management, 1908-1983," 1409
- "Die Seestadt Ancona im Spätmittelalter," by Leonhard, 1323
- Seibt, Ferdinand, editor, "Die Juden in den böhmischen Ländern" (E), 1429
- Seidel, Robert N. (R), 891
- Seidman, Steven, "Liberalism and the Origins of European Social Theory," 1297
- Seigel, Jerrold (R), 1297
- "La seigneurie bretonne, 1450-1680," by Gallet, 1332
- Seip, Terry L., "The South Returns to Congress: Men, Economic Measures, and Intersectional Relationships, 1868-1879," 525
- "The Seleucid, Parthian, and Sasanian Periods," vol. 3 of "The Cambridge History of Iran," edited by Yarshater, 1055
- Selleck, Roberta G. (R), 1097
- Sellen, Robert W. (R), 549
- Seller, Maxine Schwartz, editor, "Ethnic Theatre in the United States" (E), 578
- "Selling Art in Georgian London," by Lippincott, 1327
- Semmel, Bernard (R), 92
- Senn, Alfred Erich (R), 807
- Septimus, Bernard, "Hispano-Jewish Culture in Transition: The Career and Controversies of Ramah," 420
- SERFDOM AND THE PEASANTRY: Anfimov, "Krest'ianskoe khoziaistvo evropeiskoi Rossii, 1881-1904," 811; Atkinson, "The End of the Russian Land Commune, 1905-1930," 1119; Bokarev, "Biudzhetye obsledovaniia krest'ianskikh khoziaistv 20-kh godov kak istoricheskii istochnik," 1361; Dallas, "The Imperfect Peasant Economy," 135; Dhanagare, "Peasant Movements in India, 1920-1950," 832; Goodman and Redclift, "From Peasant to Proletarian," 1300; Prokofeva, "Krest'ianskaia obshchina v Rossii vo vtoroi polovine XVIII-pervoi polovine XIV v.," 811; Rebel, "Peasant Classes," 159; Shue, "Peasant China in Transition," 185; Thaxton, "China Turned Rightside Up," 184
- "Serving Women," by Dudden, 206
- "Servus Index," by Schumacher, 1314
- "Settlements, Trade, and Politics in the Seventeenth-Century Gold Coast," by Kea, 823
- "The Seven Liberal Arts in the Middle Ages," edited by Wagner (E), 1197
- "Seven-Headed Luther," edited by Brooks, 1346
- "Seventeenth-Century Lancashire," edited by Kermode and Phillips, 1326
- Sewell, Richard H. (R), 1159
- SEX AND SEX ROLES: Bray, "Homosexuality in Renaissance England," 421; Engel, "Mothers and Daughters," 809; Matthaei, "An Economic History of Women in America," 215; McLaren, "Sexuality and Social Order," 781; Musallam, "Sex and Society in Islam," 1363; Stafford, "Queens, Concubines, and Dowagers," 106
- "Sex and Society in Islam," by Musallam, 1363
- "Sexuality and Social Order," by McLaren, 781
- Seymour, Susan, "Anglo-Danish Relations and Germany, 1933-1945," 461
- "Shadows in the Grass," by Collins, 824
- Shafer, Byron E., "Quiet Revolution: The Struggle for the Democratic Party and the Shaping of Post-Reform Politics," 1414
- Shakespeare, William, 34
- Shapiro, Barbara (R), 474
- Shapiro, Barbara J., "Probability and Certainty in Seventeenth-Century England: A Study of the Relationships between Natural Science, Religion, History, Law, and Literature," 762
- Shapiro, Henry D. (R), 1183
- Sharlin, Harold Issadore (R), 1183
- Sharlin, Harold Issadore, editor, "Business and Its Environment: Essays for Thomas C. Cochran" (E), 253
- Sharpe, William, and Leonard Wallock, editors, "Visions of the Modern City: Essays in History, Art, and Literature" (E), 1197
- Shaw, Duncan, and Ian B. Cowan, editors, "The

- Renaissance and Reformation in Scotland: Essays in Honour of Gordon Donaldson" (E), 572
- Shaw, John Stuart, "The Management of Scottish Society, 1707–1764: Power, Nobles, Lawyers, Edinburgh Agents, and English Influences," 776
- Shaw, Stanford J. (R), 497
- Shaw, William H. (R), 1120
- "The Shawnee Prophet," by Edmunds, 848
- Shay, Robert P., Jr. (R), 775
- Shea, William L., "The Virginia Militia in the Seventeenth Century," 1145
- Sheehan, James J. (R), 1348
- Sheils, W. J., editor, "The Church and War" (E), 1195
- Sherer, Robert G. (R), 1166
- Sheridan, James E. (R), 1375
- Sherwood, Morgan (R), 102
- "Shetland and the Outside World, 1469–1969," edited by Withrington (E), 897
- Shifflett, Crandall A., "Patronage and Poverty in the Tobacco South: Louisa County, Virginia, 1860–1900," 521
- Shinnie, P. L. (R), 103
- Shirley, John W., "Thomas Harriot: A Biography," 1069
- Shlaim, Avi, "The United States and the Berlin Blockade, 1948–1949: A Study in Crisis Decision-Making," 549
- "The Shock of Peace," by Ballard, 233
- Short, K. R. M., editor, "Film and Radio Propaganda in World War II" (E), 568
- Short, Wallace M., 1171
- Shortt, S. E. D. (R), 558
- Showalter, Dennis E. (R), 1353
- Shue, Vivienne, "Peasant China in Transition: The Dynamics of Development toward Socialism, 1949–1956," 185
- Siaw, Laurence K. L., "Chinese Society in Rural Malaysia: A Local History of the Chinese in Titi, Jebeu," 1140
- Sibley, Marilyn McAdams, "Lone Stars and State Gazettes: Texas Newspapers before the Civil War," 524
- Sica, Alan (R), 1299
- Šidak, Jaroslav, "Kroz pet stoljeća Hrvatske povijesti," 1357
- Siegelbaum, Lewis H., "The Politics of Industrial Mobilization in Russia, 1914–17: A Study of the War-Industries Committees," 1360
- Siepel, Kevin H., "Rebel: The Life and Times of John Singleton Mosby," 852
- Sievers, Sharon L., "Flowers in Salt: The Beginnings of Feminist Consciousness in Modern Japan," 1137
- "El siglo XV en Castilla," by Ladero Quesada, 1088
- "Siirtoväen Pika-Asutuslaki 1940," by Hietanen, 158
- Silber, Norman Isaac, "Test and Protest: The Influence of Consumers Union," 874
- Silberstein, Gerard E. (R), 480
- Silbey, Joel H. (R), 199
- Silverman, Robert (R), 533
- Simmons, Harvey G., "From Asylum to Welfare," 558
- Simms, J. G., "William Molyneux of Dublin, 1657–1698," 129
- "Simon de Cramaud and the Great Schism," by Kaminsky, 753
- Simon, Roger D. (R), 1176
- "Simone Weil," by Hellman, 446
- Simpson, Renate, "How the PhD Came to Britain: A Century of Struggle for Postgraduate Education," 1079
- Singal, Joseph, *Beyond Consensus: Richard Hofstadter and American Historiography*, 976–1004
- Singer, Barnett, "Village Notables in Nineteenth-Century France: Priests, Mayors, Schoolmasters," 454
- Sirianni, Carmen, "Workers Control and Socialist Democracy: The Soviet Experience," 1122
- Sirianni, Carmen, and James E. Cronin, editors, "Work, Community, and Power: The Experience of Labor in Europe and America, 1900–1925" (E), 568
- Sitkoff, Harvard (R), 542; 1404
- Sivéry, Gérard, "Saint Louis et son siècle," 1324
- Skardon, Alvin W., "Steel Valley University: The Origin of Youngstown State," 1408
- Skocpol, Theda (R), 1043
- Skowronek, Stephen, "Building a New American State: The Expansion of National Administrative Capacities, 1877–1920," 213
- Skyum-Nielsen, Niels, and Niels Lund, editors, "Danish Medieval History: New Currents" (E), 249
- Slatta, Richard W., "Gauchos and the Vanishing Frontier," 1421
- SLAVERY: Berlin and Hoffman, editors, "Slavery and Freedom in the Age of the American Revolution," 512; Blackett, "Building an Antislavery Wall," 1159; Cardoso, "Negro Slavery in the Sugar Plantations of Veracruz and Pernambuco, 1550–1680," 560; Coughtry, "The Notorious Triangle," 841; Craton, "Testing the Chains," 561; Essig, "The Bonds of Wickedness," 196; Freehling, "Drift toward Dissolution," 518; Garlan, "Les esclaves en Grèce ancienne," 104; Kraut, editor, "Crusaders and Compromisers," 1158; Littlefield, "Rice and Slaves," 195; Lovejoy, "Transformations in Slavery," 820; Müller, "Die Kunst des Sklavenkaufs," 818; Patterson, "Slavery and Social Death," 411; Robertson and Klein, editors, "Women and Slavery in Africa," 1370; Rossbach, "Ambivalent Conspirators," 520; Schumacher, "Servus Index," 1314; Toledano, "The Ottoman Slave Trade and its Suppression, 1840–1890," 818
- "Slavery and Freedom in the Age of the American Revolution," edited by Berlin and Hoffman, 512
- "Slavery and Social Death," by Patterson, 411
- Slavin, Arthur J. (R), 759
- Slee, Peter R. H. (R), 1079
- "Slovak Politics," edited by Kirschbaum (E), 899
- Smith, Alfred E., 867
- Smith, Bonnie G. (R), 781
- Smith, Bonnie, *The Contribution of Women to Modern Historiography in Great Britain, France, and the United States, 1750–1940*, 709–32
- Smith, Brian H., "The Church and Politics in Chile: Challenges to Modern Catholicism," 1193
- Smith, Charles D., "Islam and the Search for Social Order in Modern Egypt: A Biography of Muhammad Husayn Haykal," 1128
- Smith, David C. (R), 1051
- Smith, Dennis, "Barrington Moore, Jr.: A Critical Appraisal," 403
- Smith, Duane A., editor, "A Taste of the West: Essays in Honor of Robert G. Athearn" (E), 578
- Smith, Henry Nash (R), 1151
- Smith, Michael S. (R), 134
- Smith, Philip E. L. (R), 738
- Smith, Robert J. (R), 138; 501
- Smith, S. A. (R), 1122
- Smith, W. Calvin (R), 842
- Smith, Wilson (R), 544
- Smith, Woodruff D. (R), 889
- Smout, T. C. (R), 447
- Snetsinger, John (R), 1384
- Snow, George E. (R), 490

- Snowden, Frank M., Jr., "Before Color Prejudice: The Ancient View of Blacks," 103
- "Social Change and the Growth of British Power in the Gold Coast," by McCarthy, 1130
- "Social Control and the State," edited by Cohen and Scull, 1305
- SOCIAL HISTORY: Arlacchi, "Mafia, Peasants, and Great Estates," 800; Asker, "Officerarna och det svenska samhället, 1650–1700," 1344; Auerbach, "Justice without Law?" 533; Ballard, "The Shock of Peace," 233; Becker, "The New Bourgeoisie and the Limits of Dependency," 891; Bennett, "Community, Class, and Careerism," 112; Bohstedt, "Riots and Community Politics in England and Wales, 1790–1810," 429; Bowen, "Rebellion and Democracy in Meiji Japan," 187; Checkland, "British Public Policy, 1776–1939," 437; Conlon, "A Caste in a Changing World," 503; Davies, "Wales in the Early Middle Ages," 416; Davis, "Good and Faithful Labor," 521; Dobbin, "Islamic Revivalism in a Changing Peasant Economy," 189; Drinkwater, "Roman Gaul," 1058; Eberhard, "Konfessionsbildung und Stände in Böhmen, 1478–1530," 1102; Estèbe, "Les ministres de la République, 1871–1914," 138; Fernández, "El mundo rural sevillano en el siglo XV," 1088; Fischer, "Stormtroopers," 470; Floristán Imízcoz, "La Merindad de Estella en la edad moderna," 1088; Friedman, "Spanish Captives in North Africa in the Early Modern Age," 1129; Gibson, "The Santa Fe and Taos Colonies," 536; Gillespie, "Soldiers of Perón," 564; Grimmer, "Vivre à Aurillac au XVIII^{ème} siècle," 1083; Haesenne-Peremans, "Les pauvres et le pouvoir," 1091; Hamerow, "The Birth of a New Europe," 736; Hanson and Maxwell, "Rome's North West Frontier," 743; Hanson, "Economy and Society in Baroque Portugal, 1668–1703," 459; Hartigan, "The Forgotten Victim," 96; Hartung, "Süddeutschland in der frühen Merowingerzeit," 1064; Henriksson, "The Tsar's Loyal Germans," 170; Hietanen, "Siirtoväen Pika-Asutuslaki 1940," 158; Himmelfarb, "The Idea of Poverty," 1073; Ho Tai, "Millenarianism and Peasant Politics in Vietnam," 508; Holmes, "Augustan England," 119; Holzer *et al.*, "The Lincoln Image," 1395; Hunter, "Science and Society in Restoration England," 763; Husung, "Protest und Repression im Vormärz," 1100; Jones, "Charity and Bienfaisance," 132; Kea, "Settlements, Trade, and Politics in the Seventeenth-Century Gold Coast," 823; Kershaw, "Popular Opinion and Political Dissent in the Third Reich," 793; Kyba, "Covenants without the Sword," 775; Leclercq, "Le mariage vu par les moines au XII^e siècle," 1062; Lewis, "Life in Egypt under Roman Rule," 1058; Lloyd, "The State, France, and the Sixteenth Century," 130; MacLeod, "Building Character in the American Boy," 1169; McCarthy, "Social Change and the Growth of British Power in the Gold Coast," 1130; McClintock and Lowenthal, editors, "The Peruvian Experiment Reconsidered," 891; Mizruchi, "Regulating Society," 410; Mulvey, "Anglo-American Landscapes," 93; Niethammer, editor, "Die Jahre weiss man nicht, wo man die heute hinsetzen soll," 795; Nipperdey, "Deutsche Geschichte, 1800–1866," 1099; Nobles, "Divisions throughout the Whole," 1146; Offen, *Depopulation, Nationalism, and Feminism in Fin-de-Siècle France*, 648–76; Owen, "Capitalism and Politics in Russia," 1117; Palliser, "The Age of Elizabeth," 423; Prude, "The Coming of Industrial Order," 1157; Rice, "Early American Taverns," 1145; Rooke and Schnell, "Discarding the Asylum," 1415; Rothman, "Conscience and Convenience," 227; Saad, "Social History of Timbuktu," 1371; Şandru, "Populația rurală a României între cele două războaie mondiale," 163; Schweider, "Black Diamonds," 229; Shiflett, "Patronage and Poverty in the Tobacco South," 521; Simmons, "From Asylum to Welfare," 558; Singer, "Village Notables in Nineteenth-Century France," 454; Sokal, *The Gestalt Psychologists in Behaviorist America*, 1240–63; Strauss, "Traitors to the Masculine Cause," 97; Suppan, "Die österreichischen Volksgruppen," 1104; Surratt, "Gottlieb Schober of Salem," 842; Thaxton, "China Turned Rightside Up," 184; Walton, "The English Seaside Resort," 443; Wayne, "The Reshaping of Plantation Society," 521; Weir, "Colonial South Carolina," 511; Weisser, "April 10," 430
- "Social History of Timbuktu," by Saad, 1371
- "Social Justice and Church Authority," by Bronder, 222
- "Social Relations and Ideas," edited by Aston *et al.* (E), 241
- "The Social Transformation of American Medicine," by Starr, 532
- "Social Welfare or Social Control?" edited by Trattner (E), 571
- "Socialism in Galicia," by Himka, 1114
- SOCIALISM: Buhle, "Women and American Socialism, 1870–1920," 212; Himka, "Socialism in Galicia," 1114; Lindemann, "A History of European Socialism," 92; Nash, "Conflict and Accommodation," 1164; Rabinbach, "The Crisis of Austrian Socialism," 160; Redžić, "Jugoslovenska misao i socijalizam," 804; Taylor, "Eve and the New Jerusalem," 121; Wittig, "Der englische Weg zum Sozialismus," 770
- "Society in Change," edited by Vardy and Vardy (E), 569
- SOCIOLOGY: Abrams, "Historical Sociology," 1298; Allen, "The Romance of Commerce and Culture," 1409; Calvert, "The Concept of Class," 98; Cohen and Scull, editors, "Social Control and the State," 1305; Finsch, "Die Goldgräber Kaliforniens," 206; Kern, "Empirische Sozialforschung," 142; Slatta, "Gauchos and the Vanishing Frontier," 1421
- Soffer, Reba N. (R), 445
- Sokal, Michael M., *The Gestalt Psychologists in Behaviorist America*, 1240–63
- Solberg, Carl E. (R), 892
- "Soldiers of Perón," by Gillespie, 564
- Soliday, Gerald L. (R), 1349
- Solnick, Bruce B. (R), 887
- Solt, Leo F. (R), 426
- "Some Arval Brethren," by Syme, 414
- Sommerville, C. John, "The Rise and Fall of Childhood," 95
- "Song and Democratic Culture in Britain," by Watson, 1081
- Sonne, Conway B., "Saints on the Seas: A Maritime History of Mormon Migration, 1830–1890," 851
- Sonnichsen, C. L. (R), 536
- "Sons of Liberty," by Pugh, 1382
- "A Sort of Utopia," by O'Connor, 228
- Sosin, J. M. (R), 512; 735
- "Sotsial'no-ekonomicheskii stroi pomeschchich'ego khoziaistva Evropeiskoi Rossii v epokhu kapitalizma," by Koval'chenko *et al.*, 170
- "Le Soudan nilotique et l'administration britannique, 1898–1956," by Grandin, 124
- "The South Returns to Congress," by Seip, 525

- "A Southern Community in Crisis," by Campbell, 1397
- "The Southern Enigma," edited by Fraser and Moore (E), 1201
- "Southern Progressivism," by Grantham, 1403
- "A Southern Rebel," by Salmond, 224
- "Soviet-East European Relations," by Hutchings, 1362
- Sowerwine, Charles (R), 136
- "Soziale Mobilität und Chancengleichheit im 19. und 20. Jahrhundert," by Kaelble, 1302
- "Soziale Ungleichheit vor Krankheit und Tod," by Spree, 790
- SPACE EXPLORATION AND ASTRONOMY: Webb, "Tree Rings and Telescopes," 534
- "The Space of Death," by Ragon, 1307
- Spahr, William J. (R), 494
- "Spain and the Loss of America," by Anna, 1090
- "Spaniards and Indians in Southeastern Mesoamerica," edited by MacLeod and Wasserstrom (E), 1432
- "Spanish Captives in North Africa in the Early Modern Age," by Friedman, 1129
- "The Spanish Civil War," by Beevor, 1090
- Spate, O. H. K., "The Pacific since Magellan," vol. 2, "Monopolists and Freebooters," 734
- Spear, Allan H. (R), 542
- Spector, Ronald H., "Advice and Support: The Early Years, 1941-1960," 1411
- Sperlich, Waltraud, "Journalist mit Mandat: Sozialdemokratische Reichstagsabgeordnete und ihre Arbeit in der Parteipresse, 1867 bis 1918," 1350
- "Spezialforschung und 'Gesamtgeschichte,'" edited by Klingenstein and Lutz (E), 246
- Spiegel, Gabrielle M. (R), 1324
- Spiers, Edward M., "Radical General: Sir George de Lacy Evans, 1787-1870," 1071
- Spinner, Thomas J., Jr. (R), 432
- "Die Spitzenverbände der Wirtschaft und das erste Kabinett Brüning," by Grübler, 149
- Spitzer, Alan B. (R), 783
- Spivakovsky, Erika (R), 798
- Spooner, Frank C., "Risks at Sea: Amsterdam Insurance and Maritime Europe, 1766-1780," 1092
- "Sport and American Mentality, 1880-1910," by Mrozek, 1168
- SPORTS AND LEISURE ACTIVITIES: Guttman, "The Games Must Go On," 1309; Mrozek, "Sport and American Mentality, 1880-1910," 1168; Tygiel, "Baseball's Great Experiment," 877; Walton, "The English Seaside Resort," 443
- Sprandel, Rolf, "Altersschicksal und Altersmoral: Die Geschichte der Einstellungen zum Altern nach der Pariser Bibelexegese des 12.-16. Jahrhunderts," 1060
- Spree, Reinhard, "Soziale Ungleichheit vor Krankheit und Tod: Zur Sozialgeschichte des Gesundheitsbereichs im Deutschen Kaiserreich," 790
- Spring, David (R), 733
- Sprunger, Keith L., "Dutch Puritanism: A History of English and Scottish Churches of the Netherlands in the Sixteenth and Seventeenth Centuries," 788
- "Staatsräson und christliche Solidarität," by Köpeczi, 1111
- Stackelberg, Roderick (R), 151
- "Städtewesen und Merkantilismus in Mitteleuropa," edited by Press, 1349
- Stafford, Pauline, "Queens, Concubines, and Dowagers: The King's Wife in the Early Middle Ages," 106
- Stagg, J. C. A., "Mr. Madison's War: Politics, Diplomacy, and Warfare in the Early American Republic, 1783-1830," 1153
- Stanislawski, Michael, "Tsar Nicholas I and the Jews: The Transformation of Jewish Society in Russia, 1825-1855," 810
- Stanley, George F. G., "The War of 1812: Land Operations," 1153
- "The Stanleys," by Coward, 1071
- Stapeldon, Walter, 1322
- Starr, Chester G. (R), 98
- Starr, Paul, "The Social Transformation of American Medicine," 532
- Starr, S. Frederick, "Red and Hot: The Fate of Jazz in the Soviet Union, 1917-1980," 174
- "State and Diplomacy in Early Modern Japan," by Toby, 1375
- "State and Government in Medieval Islam," by Lambton, 1125
- "State and Society in Contemporary China," edited by Nee and Mozingo (E), 577
- "State and Society in Roman Galilee, A.D. 132-212," by Goodman, 1315
- "The State in Capitalist Europe," edited by Bornstein *et al.* (E), 1428
- "The State, France, and the Sixteenth Century," by Lloyd, 130
- "States of Mind," by MacDonagh, 778
- "La statistique en France à l'époque napoléonienne," by Bergeron *et al.* (E), 246
- Stearns, Peter N. (R), 406
- Steel, Ronald (C), 1223
- "Steel Valley University," by Skardon, 1408
- Steinberg, Michael Stephen (R), 470
- Stephen, James Fitzjames, 122
- "Stephen Sayre," by Alden, 1147
- Stephens, Lester D. (R), 1038
- Stephens, W. B., editor, "Studies in the History of Literacy: England and North America" (E), 895
- "Stéréotypes nationaux et préjugés raciaux aux XIX^e et XX^e siècles," edited by Pirotte (E), 241
- Stern, M. S. (R), 1365
- Stevens, Edward (R), 202
- Stevens, Robert, "Law School: Legal Education in America from the 1850s to the 1980s," 1398
- Stevens, Wesley M. (R), 1060
- Stevenson, D., "French War Aims against Germany, 1914-1919," 456
- Stevenson, John (R), 447
- Stevenson, John, and Michael Bentley, editors, "High and Low Politics in Modern Britain: Ten Studies" (E), 896
- Stewart, Peter C., and Richard A. Rutyna, editors, "Virginia in the American Revolution: A Collection of Essays" (E), 253
- Stewart-McDougall, Mary Lynn (R), 1336
- Stiefel, Tina (R), 748
- Stilgoe, John R., "Metropolitan Corridor: Railroads and the American Scene," 1174
- Still, William N., Jr., "American Sea Power in the Old World: The United States Navy in European and Near Eastern Waters, 1865-1917," 517
- "Stillborn Republic," by Mavrogordatos, 162
- Stinger, Charles L. (R), 1108
- Stites, Richard (R), 493; 814
- Stock, Brian, "The Implications of Literacy: Written Language and Models of Interpretation in the Eleventh and Twelfth Centuries," 107
- Stockwell, A. J. (R), 836
- Stoetzer, O. Carlos (R), 1419
- "Stormtroopers," by Fischer, 470
- "Storskiftet i Ekebyborna," by Olai, 462

- Stover, John F. (R), 1410
 Strauss, Sylvia, "'Traitors to the Masculine Cause': The Men's Campaigns for Women's Rights," 97
 Strong, Douglas H. (R), 223
 "Stronger than a Hundred Men," by Reynolds, 101
 Strozier, Charles B. (R), 867
 "The Struggle for Land," by Foweraker, 1193
 Struthers, James, "No Fault of Their Own: Unemployment and the Canadian Welfare State, 1914–1941," 557
 Struve, Walter, "Die Republik Texas, Bremen und das Hildesheimische: Ein Beitrag zur Geschichte von Auswanderung, Handel und gesellschaftlichem Wandel im 19. Jahrhundert, mit den Briefen eines deutschen Kaufmans und Landwirts in Texas, 1844–1845," 1155
 Stuard, Susan Mosher (R), 747
 Stuart, Reginald C. (R), 197
 Stubbe, Henry, 426
 "Studies in Numismatic Method," edited by Brooke *et al.* (E), 569
 "Studies in the History of Literacy," edited by Stephens (E), 895
 "Studies in Tudor and Stuart Politics and Government," vol. 3, "Papers and Reviews, 1973–1981," by Elton (E), 896
 "Studies on the Political Thought of Gaetano Mosca," edited by Albertoni (E), 248
 Sturma, Michael, "Vice in a Vicious Society: Crime and Convicts in Mid-Nineteenth Century New South Wales," 1142
 Sturtevant, William C. (R), 1380
 Stutsman, Enos, 1415
 "Subaltern Studies," vol. 2, "Writings on South Asian History and Society," edited by Guha (E), 1430
 "Subject to Famine," by McAlpin, 1139
 "Süddeutschland in der frühen Merowingerzeit," by Hartung, 1064
 "Suetonius," by Wallace-Hadrill, 1314
 Sugar, Peter F. (R), 486
 Suggs, Henry Lewis, editor, "The Black Press in the South, 1865–1979" (E), 1202
 "Sul Ross," by Benner, 1162
 Suleiman, Ezra N. (R), 784
 Sullivan, Antony Thrall, "Thomas-Robert Bugeaud: France and Algeria, 1784–1849: Politics, Power, and the Good Society," 133
 Summerville, James, "Educating Black Doctors: A History of Meharry Medical College," 1166
 Sunseri, Alvin R. (R), 209
 Suny, Ronald Grigor, editor, "Transcaucasia: Nationalism and Social Change: Essays in the History of Armenia, Azerbaijan, and Georgia" (E), 1201
 Super, John C. (R), 238
 Suppan, Arnold, "Die österreichischen Volksgruppen: Tendenzen ihrer gesellschaftlichen Entwicklung im 20. Jahrhundert," 1104
 "The Supply of Schooling," edited by Frijhoff (E), 569
 Surratt, Jerry L., "Gottlieb Schober of Salem: Discipleship and Ecumenical Vision in an Early Moravian Town," 842
 "Survivor," by Fingerhut, 1388
 Sutcliffe, Anthony, and Derek Fraser, editors, "The Pursuit of Urban History" (E), 568
 Sutcliffe, Anthony, editor, "Metropolis, 1890–1940" (E), 1426
 Sutherland, Daniel E. (R), 206
 Sutherland, Donald, "The Chouans: The Social Origins of Popular Counter-Revolution in Upper Brittany, 1770–1796," 780
 Sutherland, Neil (R), 1415
 Sutton, Donald S. (R), 183
 Sutton, Michael, "Nationalism, Positivism, and Catholicism: The Politics of Charles Maurras and French Catholics, 1890–1914," 140
 Sweet, David G. (R), 560
 Sweet, Leonard I., "The Minister's Wife: Her Role in Nineteenth-Century American Evangelicalism," 516
 Sweet, William Warren, 222
 Sweets, John F. (R), 1340
 Swietochowski, Tadeusz (R), 1114
 Sydorenko, Alexander (R), 808
 Symcox, Geoffrey, "Victor Amadeus II: Absolutism in the Savoyard State, 1675–1730," 1355
 Syme, Ronald, "Some Arval Brethren," 414
 Symonds, Craig L. (R), 1154
 "Synkritiki Istoría Ellados kai Tourkias stou 20% aiona," by Kitsikis, 803
 Szasz, Ferenc Morton, "The Divided Mind of Protestant America, 1880–1930," 861
 "Széchenyi Döblingben," by Kosáry, 164
 Széchenyi, István, 164
 Szeftel, Marc M. (R), 812
 Szemler, G. J. (R), 739
 Szporluk, Roman (R), 484
 Suchman, Mark D., "Mobility and Integration in Urban Argentina: Córdoba in the Liberal Era," 1194
 Taagepera, Rein, and Romuald J. Misiunas, "The Baltic States: Years of Dependence, 1940–1980," 807
 Tachau, Frank (R), 176
 "Tacitus' germanische Gefolgschaft," by Kristensen, 743
 Takaki, Ronald (R), 1160
 Takaki, Ronald, "Pau Hana: Plantation Life and Labor in Hawaii, 1835–1920," 857
 "A Tale of Two Agencies," by Mosher, 1412
 Talmage, Frank (R), 1064
 Tanis, James (R), 788
 Tanović, Arif, *et al.*, editors, "O djelu Veselina Masleše" (E), 1200
 Tao, Chia-Lin Pao (R), 826
 Tarling, Nicholas, "The Burthen, the Risk, and the Glory: A Biography of Sir James Brooke," 837
 "Tarnished Expansion," by Holbo, 526
 "A Taste of the West," edited by Smith (E), 578
 Taylor, A. Elizabeth (R), 196
 Taylor, Anne, "Laurence Oliphant, 1829–1888," 121
 Taylor, Barbara, "Eve and the New Jerusalem: Socialism and Feminism in the Nineteenth Century," 121
 Taylor, George V. (R), 779
 Taylor, Keith Weller, "The Birth of Vietnam," 834
 Taylor, Philip M., and M. L. Sanders, "British Propaganda during the First World War, 1914–18," 1081
 Taylor, William B. (R), 1189
 Tazewell, Littleton Waller, 1394
 Tebbutt, Melanie, "Making Ends Meet: Pawnbroking and Working-Class Credit," 433
 TECHNOLOGY: Friedel, "Pioneer Plastic," 1183; Horwitch, "Clipped Wings," 553; Hughes, "Networks of Power," 406; Kern, "The Culture of Time and Space, 1880–1918," 736; Koppes, "JPL and the American Space Program," 544; Landes, "Revolution in Time," 1052; Piggott, "The Earliest Wheeled Transport," 738; Reynolds, "Stronger than a Hundred Men," 101
 Teichova, Alice (R), 1104

- "Tell about the South," by Hobson, 1381
 Tello, Carlos, and Clark W. Reynolds, editors,
 "U.S.-Mexico Relations: Economic and Social
 Aspects" (E), 901
 Teng, S. Y. (R), 500
 Tent, James F., "Mission on the Rhine: Reeducation
 and Denazification in American-Occupied
 Germany," 154
 Tepaske, John J. (R), 559
 "Terrorists and Social Democrats," by Naimark, 489
 "Test and Protest," by Silber, 874
 "Testing the Chains," by Craton, 561
 Thaden, Edward C. (R), 814
 Thaxton, Ralph, "China Turned Rightside Up:
 Revolutionary Legitimacy in the Peasant World,"
 184
 THEATER: Cone, "First Rival of the Metropolitan
 Opera," 1162
 "Theft in Athenian Law," by Cohen, 1057
 "Their Number Become Thinned," by Dobyns, 1380
 "Theodor Herzl: Briefe und autoiographische
 Notizen, 1866-1895," vol. 1 of "Theodor Herzl:
 Briefe und Tagebücher," edited by Wachten, 1111
 "Theodor Herzl: Briefe und Tagebücher," vol. 1,
 "Theodor Herzl: Briefe und autoiographische
 Notizen, 1866-1895," edited by Wachten, 1111
 "Theodosian Empresses," by Holm, 415
 "They Called Them Greasers," by De León, 857
 "The Third Reformation?" by Lindberg, 1040
 "This Land, This South," by Cowdrey, 839
 "Thomas Chalmers and the Godly Commonwealth in
 Scotland," by Brown, 447
 Thomas, Daniel H., "The Guarantee of Belgian
 Independence and Neutrality in European
 Diplomacy, 1830's-1930's," 1342
 "Thomas Eakins," by Johns, 1398
 "Thomas Harriot," by Shirley, 1069
 Thomas, Keith, "Man and the Natural World: A
 History of the Modern Sensibility," 733
 Thomas, Lewis H., 254
 "Thomas More," by Fox, 422
 "Thomas R. R. Cobb (1823-1862)," by McCash, 1392
 "Thomas-Robert Bugeaud," by Sullivan, 133
 Thompson, Gerald, "Edward F. Beale and the
 American West," 852
 Thompson, Kenneth W., "Winston Churchill's World
 View: Statesmanship and Power," 125
 Thompson, Kenneth W., editor, "Essays on Lincoln's
 Faith and Politics" (E), 1432
 Thompson, Leonard (R), 1132
 Thompson, Paul (R), 433
 Thomson, J. K. J. (R), 131
 Thomson, William P. L., "The Little General and the
 Rousay Crofters: Crisis and Conflict on an Orkney
 Crofting Estate," 448
 Thornton, John K., "The Kingdom of Kongo: Civil
 War and Transition, 1641-1718," 1130
 Thrower, James, "Marxist-Leninist 'Scientific Atheism'
 and the Study of Religion and Atheism in the
 USSR," 817
 Thuillier, Guy, "La monnaie en France au début du
 XIX^e siècle," 1335
 "Tiden 1730-1814," vol. 4 of "Danmarks historie," by
 Feldbæk, 461
 Tignor, Robert L. (R), 124
 Tikas, Louis, 229
 Tillett, Ben, 445
 Tilly, Richard H. (R), 792
 "Timothy Pickering and the Age of the American
 Revolution," by McLean, 514
 Tiushkevich, S. A., *et al.*, "Istoriia vtoroi mirovoi
 voiny, 1939-1945," vol. 12, "Itogi i uroki vtoroi
 mirovoi voiny," 494
 Tiutiukin, S. V., "Pervaia rossiiskaia revoliutsiia i G.
 V. Plekhanov: Iz istorii ideinoi bor'by v rabochem
 dvizhenii Rossii v 1905-1907 gg.," 172
 Tjerneld, Andreas, "Fråan borgarståndets
 storhetstid: Statsbudgeten som partiskiljande fråga
 i den sena ståndsriksdagen," 1095
 "To Set a Standard of Workmanship and Compel Men to
 Conform to it": John Franklin Jameson as Editor of the
 American Historical Review, by Rothberg, 957-75
 Tober, James A. (R), 1180
 Tober, James A., "Who Owns the Wildlife? The
 Political Economy of Conservation in Nineteenth-
 Century America," 855
 Toby, Ronald P., "State and Diplomacy in Early
 Modern Japan: Asia in the Development of the
 Tokugawa Bakufu," 1375
 "Tokushii Yoron," by Hakuseki, 186
 Toledano, Ehud R., "The Ottoman Slave Trade and
 its Suppression, 1840-1890," 818
 Tomás y Valiente, Francisco, "Los validos en la
 monarquía española del siglo XVII: Estudio
 institucional," 786
 Tomes, Nancy J. (R), 548
 Tompson, Richard (R), 116
 Toohey, Robert E. (R), 119
 "Torn between Two Lands," by Mirak, 1401
 Totman, Conrad (R), 1375
 "Towards the Holocaust," edited by Dobkowski and
 Wallimann (E), 573
 Tracy, James D. (R), 1098
 "Trade and Aid," by Kaufman, 550
 "Trade and Crusade," by Zachariadou, 1126
 "Trade Unionism in Germany from Bismarck to
 Hitler, 1869-1933," by Moses, 147
 Traill-Burroughs, Frederick William, 448
 "'Traitors to the Masculine Cause,'" by Strauss, 97
 "Transcaucasia," edited by Suny (E), 1201
 "The Transformation of Higher Learning,
 1860-1930," edited by Jarausch (E), 570
 "The Transformation of Political Culture," by
 Formisano, 200
 "Transformations in Slavery," by Lovejoy, 820
 "La transizione dall'economia di guerra all'economia
 di pace in Italia e in Germania dopo la Prima
 guerra mondiale," edited by Hertner and Mori (E),
 575
 TRANSPORTATION: Barrett, "The Automobile and
 Urban Transit," 1172; Corn, "The Winged
 Gospel," 876; Gillispie, "The Montgolfier Brothers
 and the Invention of Aviation, 1783-1784," 1052;
 Kurgan-van Hentenryk, "Rail, finance, et
 politique," 787; Piggott, "The Earliest Wheeled
 Transport," 738; Robertson, "The Origins of the
 Scottish Railway System, 1722-1844," 777; Stilgoe,
 "Metropolitan Corridor," 1174
 Trattner, Walter I. (R), 1169
 Trattner, Walter I., editor, "Social Welfare or Social
 Control? Some Historical Reflections on *Regulating
 the Poor*" (E), 571
 Trauth, Mary Philip (R), 1357
 Treadgold, Warren T., "The Byzantine State
 Finances in the Eighth and Ninth Centuries," 110
 Treadway, John D., "The Falcon and the Eagle:
 Montenegro and Austria-Hungary, 1908-1914,"
 480
 "Tree Rings and Telescopes," by Webb, 534
 Trexler, Richard C., "Public Life in Renaissance
 Florence," 475
 "Tribe and State in Bahrain," by Khuri, 1368
 "The Triumph of Sectionalism," by Maizlish, 1158

- Troeltsch, Ernst, 791
 Troper, Harold, and Irving Abella, "None is Too Many: Canada and the Jews of Europe, 1933-1948," 886
 Troxell, Hyla A., "The Coinage of the Lycian League," 1313
 "True Sisterhood," by Motz, 1383
 "Tsar Nicholas I and the Jews," by Stanislawski, 810
 "The Tsar's Loyal Germans," by Henriksson, 170
 "Tsentral'nyi sovet fabzavkomov Petrograda, 1917-1918 gg.," by Blinov, 817
 Tucker, Nancy Bernkopf, "Patterns in the Dust: Chinese-American Relations and the Recognition Controversy, 1949-1950," 1180
 Tulchin, Joseph S. (R), 1194
 Turner, Frank M. (R), 441
 Turner, James (R), 93
 Turner, Lynn Warren, "The Ninth State: New Hampshire's Formative Years," 847
 "Twentieth Century," vol. 2 of "History of the Balkans," by Jelavich, 1116
 Twersky, Isadore, editor, "Rabbi Moses Nahmanides (Ramban): Explorations in His Religious and Literary Virtuosity" (E), 1196
 Twilight, Ben W., "Organizational Values and Political Power: The Forest Service Versus the Olympic National Park," 1181
 "Twilight of the Comintern, 1930-1935," by Carr, 815
 "Two if by Sea," by Browning, 1154
 Tyack, David, and Elisabeth Hansot, "Managers of Virtue: Public School Leadership in America, 1820-1980," 531
 Tygiel, Jules, "Baseball's Great Experiment: Jackie Robinson and His Legacy," 877
 "Typologies in England, 1650-1820," by Korshin, 116
- Ulam, Adam B., "Dangerous Relations: The Soviet Union in World Politics, 1970-1982," 174
 Underdown, David (R), 117
 "Ungarn und Österreich unter Maria Theresia und Joseph II.," edited by Drabek *et al.* (E), 250
 "The Union of Lublin," by Dembkowski, 483
 "Unionsparteien, Sozialdemokratie und Vereinigte Staaten von Amerika, 1945-1966," by Grabbe, 155
 "The United Brotherhood of Carpenters," by Galenson, 1400
 "The United States and the Berlin Blockade, 1948-1949," by Shlaim, 549
 "United States Oil Policy and Diplomacy," by Chester, 884
 "United States-Iranian Relations," by Grayson, 885
 "U.S.-Korean Relations, 1882-1982," edited by Kwak *et al.* (E), 1202
 "U.S.-Mexico Relations," edited by Reynolds and Tello (E), 901
 "Ur Nordisk kulturhistoria," edited by Jokipii and Nummela (E), 249
 "Urban and Rural Change in West Germany," edited by Wild (E), 899
 "Urban Growth in Colonial Rhode Island," by Withey, 1386
 URBAN HISTORY: Barrett, "The Automobile and Urban Transit," 1172; Boyer, "Dreaming the Rational City," 1176; Chevalier, "Les bonnes villes de France du XIV^e au XVI^e Siècle," 753; Englander, "Landlord and Tenant in Urban Britain, 1838-1918," 769; Hartog, "Public Property and Private Power," 1149; Hinojosa, "A Borderlands Town in Transition," 1150; Mollenkopf, "The Contested City," 1175; Platt, "City Building in the New South," 209; Press, editor, "Städtewesen und Merkantilismus in Mitteleuropa," 1349; Quiring-Zoche, "Isfahan im 15. und 16. Jahrhundert," 499; Rigaudière, "Saint-Flour ville d'Auvergne au bas Moyen Age," 753; Romo, "East Los Angeles," 544; Sandhu and Wheatley, editors, "Melaka," 835; Szuchman, "Mobility and Integration in Urban Argentina," 1194; Withey, "Urban Growth in Colonial Rhode Island," 1386
 "Urban Workers and Labor Unions in Chile, 1902-1927," by De Shazo, 892
 "The Urge to Mobilize," by Yaney, 1118
 Urofsky, Melvin I., "A Voice That Spoke for Justice: The Life and Times of Stephen S. Wise," 225
 "The Uses of Science in the Age of Newton," edited by Burke (E), 896
- Vale, Juliet, "Edward III and Chivalry: Chivalric Society and Its Context, 1270-1350," 749
 "Los validos en la monarquía española del siglo XVII," by Tomás y Valiente, 786
 Valkenier, Elizabeth Kridl (R), 94
 Valsecchi, Franco, and Rudolf Lill, editors, "Il nazionalismo in Italia e in Germania fino alla Prima guerra mondiale" (E), 1196
 "Values and Assumptions in American Labor Law," by Atleson, 546
 van Aytta, Viglius, 1343
 Van Buren, Martin, 199
 Van der Wee, Herman (R), 789
 "Van Diemen's Land from the Earliest Times to 1855," vol. 1 of "A History of Tasmania," by Robson, 1143
 Van Dijk, C., "Rebellion under the Banner of Islam: The Darul Islam in Indonesia," 190
 Van Dülmen, Richard, editor, "Kultur der einfachen Leute: Bayerisches Volksleben vom 16. bis zum 19. Jahrhundert" (E), 572
 Van Seters, John, "In Search of History: Historiography in the Ancient World and the Origins of Biblical History," 412
 Van Tassel, David D., *From Learned Society to Professional Organization: The American Historical Association, 1884-1900*, 929-56
 van Velthoven, Harry, "De Vlaamse Kwestie, 1830-1914: Macht en Onmacht van de Vlaamsgezindheden," 1341
 "Vang og tægt," by Frandsen, 1093
 Vann, James Allen (R), 463
 Vann, James Allen, "The Making of a State: Württemberg, 1593-1793," 1347
 Vardy, Agnes Huszar, and Steven Bela Vardy, editors, "Society in Change: Studies in Honor of Béla K. Király" (E), 569
 Vardy, Steven Bela, and Agnes Huszar Vardy, editors, "Society in Change: Studies in Honor of Béla K. Király" (E), 569
 Varg, Paul A. (R), 537
 Varg, Paul A., "New England and Foreign Relations, 1789-1850," 197
 "Variations in Business and Economic History," edited by Dalgaard and Vedder (E), 242
 Vasquez, John A. (R), 1046
 Vasquez, John A., "The Power of Power Politics: A Critique," 1046
 Vassberg, David E. (R), 1088
 Vassberg, David E., "La venta de tierras baldías: El

- comunitarismo agrario y la corona de Castilla durante el siglo XVI," 1088
- Vedder, Richard K., and Bruce R. Dalgaard, editors, "Variations in Business and Economic History: Essays in Honor of Donald L. Kemmerer" (E), 242
- "Venedig und das Reich," by Rösch, 115
- "Venezuela und Deutschland, 1815–1870," by Walter, 889
- Venohr, Wolfgang, and Sebastian Haffner, "Profils Prussiens" (E), 898
- "La venta de tierras baldías," by Vassberg, 1088
- "Vera Zasulich," by Bergman, 814
- "Die verdrängte Niederlage," by Heinemann, 1352
- Verhelst, D., and W. Lourdaux, editors, "Benedictine Culture, 750–1050," 1318
- Vermes, Gabor P. (R), 1112
- Veyrassat, Béatrice, "Négociants et fabricants dans l'industrie contonnière suisse, 1760–1840: Aux origines financières de l'industrialisation," 797
- Veytia, Mariano, 1188
- "Vice in a Vicious Society," by Sturma, 1142
- "Victor Amadeus II," by Symcox, 1355
- "A Victorian Authority," by Rutherford, 237
- "Victorian Dress in Photographs," by Ginsburg, 434
- "The Victorian Girl and the Feminine Ideal," by Gorham, 1329
- "Vietnam as History," edited by Braestrup (E), 1202
- "Viglius van Aytta als humanist en diplomaat, 1507–1549," by Postma, 1343
- "Viktorianisches England in deutscher Perspektive," edited by Birke and Kluxen (E), 1198
- "Village Notables in Nineteenth-Century France," by Singer, 454
- Vinten-Johansen, Peter (R), 1094
- Viola, Herman J. (R), 848
- "Virginia in the American Revolution," edited by Rutyna and Stewart (E), 253
- "The Virginia Militia in the Seventeenth Century," by Shea, 1145
- Viscount Morpeth (George William Frederick Howard), 432
- "Visions of the Modern City," edited by Sharpe and Wallock (E), 1197
- "Vivre à Aurillac au XVIII^{ème} siècle," by Grimmer, 1083
- "De Vlaamse Kwestie, 1830–1914," by van Velthoven, 1341
- Vogler, Bernard, and Georges Livet, editors, "Pouvoir, ville et société en Europe, 1650–1750" (E), 567
- "A Voice That Spoke for Justice," by Urofsky, 225
- "Voices of Resurgent Islam," edited by Esposito (E), 1201
- "Volkskapitalisme," by O'Meara, 1132
- Volpi, Giuseppe, 800
- "Vom alten Reich zu neuer Staatlichkeit," edited by Gerlich (E), 247
- "Vom Auswanderungsland zum Einwanderungsland?" by Bade, 795
- "Vom Frankenreich zur Entfaltung Deutschlands und Frankreichs," by Werner (E), 1428
- "Von der Kapitulation bis zum Plevn-Plan," vol. 1 of "Anfänge westdeutscher Sicherheitspolitik, 1945–1956," by Foerster *et al.*, 472
- Von Falkenstein, Eugénie Trützschler, "Der Kampf der Tschechen um die historischen Rechte der böhmischen Krone im Spiegel der Presse, 1861–1879," 166
- von Kondratowitz, Hans-Joachim, and Christoph Conrad, editors, "Gerontologie und Sozialgeschichte: Wege zu einer historischen Betrachtung des Alters" (E), 898
- von Kothenin, Casimir, 1097
- Von Pistohlkors, Gert, and Andrew Ezergailis, editors, "The Russian Baltic Provinces between the 1905/1917 Revolutions" (E), 251
- "Voprosy metodologii istoricheskogo issledovaniia," by Cherepnin, 486
- "Voting in Revolutionary America," by Dinkin, 197
- Vovelle, Michel, "La mort et l'Occident de 1300 à nos jours," 1307
- "'Vseobshchaia istoriia prava' v russkom dorevoliutsionnom pravovedenii, XIX vek," by Emel'ianova, 488
- Vucinich, Wayne S. (R), 1357
- Vucinich, Wayne S., editor, "At the Brink of War and Peace: The Tito-Stalin Split in a Historic Perspective," 481
- Vucinich, Wayne S., editor, "The First Serbian Uprising, 1804–1813," 486
- Vyverberg, Henry (R), 1305
- "Wachstum, Krisen, Handlungsspielräume der Wirtschaftspolitik," by Borchardt, 465
- Wachten, Johannes, editor, "Theodor Herzl: Briefe und Tagebücher," vol. 1, "Theodor Herzl: Briefe und autoiographische Notizen, 1866–1895," 1111
- Wagner, David L., editor, "The Seven Liberal Arts in the Middle Ages" (E), 1197
- Wah, Chin Kin, "The Defence of Malaysia and Singapore: The Transformation of a Security System, 1957–1971," 836
- Wakelyn, Jon L., and Randall M. Miller, editors, "Catholics in the Old South: Essays on Church and Culture," 850
- Wald, Kenneth D., "Crosses on the Ballot: Patterns of British Voter Alignment since 1885," 436
- "Wales in the Early Middle Ages," by Davies, 416
- Walicki, Andrzej, "Philosophy and Romantic Nationalism: The Case of Poland," 484
- Walker, Juliet E. K., "Free Frank: A Black Pioneer on the Antebellum Frontier," 1396
- Walker, Lawrence D. (R), 1306
- Wall, Irwin M., "French Communism in the Era of Stalin: The Quest for Unity and Integration, 1945–1962," 1087
- Wall, Richard, *et al.*, editors, "Family Forms in Historic Europe" (E), 244
- Wallace, Alfred Russell, 1297
- Wallace, Ernest (R), 524
- "Wallace M. Short," by Cumberland, 1171
- Wallace-Hadrill, Andrew, "Suetonius: The Scholar and His Caesars," 1314
- Waller, Robert J., "The Dukeries Transformed: The Social and Political Development of a Twentieth-Century Coalfield," 1330
- Wallimann, Isidor, and Michael N. Dobkowski, editors, "Towards the Holocaust: The Social and Economic Collapse of the Weimar Republic" (E), 573
- Wallock, Leonard, and William Sharpe, editors, "Visions of the Modern City: Essays in History, Art, and Literature" (E), 1197
- Walsh, James P. (R), 523
- Walsh, Judith E., "Growing Up in British India: Indian Autobiographers on Childhood and Education under the Raj," 831
- Walsh, Margaret, "The Rise of the Midwestern Meat Packing Industry," 529
- "Walter Reuther and the Rise of the Auto Workers," by Barnard, 227
- Walter, Richard J. (R), 1420

- Walter, Rolf, "Venezuela und Deutschland, 1815–1870," 889
- Walton, John K., "The English Seaside Resort: A Social History, 1750–1914," 443
- Wanandi, Jusuf, and Robert A. Scalapino, editors, "Economic, Political, and Security Issues in Southeast Asia in the 1980s" (E), 577
- "War and Society in East Central Europe," vol. 13, "The Effects of World War I," edited by Banac (E), 575
- "War in the Modern Great Power System, 1495–1975," by Levy, 1046
- "The War of 1812," by Stanley, 1153
- "War, Revolution, and Society in Romania," edited by Ceausescu (E), 576
- Ward, Patricia C., and W. Peter Ward, *Infant Birth Weight and Nutrition in Industrializing Montreal*, 324–45
- Ward, W. Peter, and Patricia C. Ward, *Infant Birth Weight and Nutrition in Industrializing Montreal*, 324–45
- Warmbrum, Werner (R), 460
- Warnicke, Retha M., "Women of the English Renaissance and Reformation," 421
- Warren, Jim (R), 509
- "The Warrior and the Priest," by Cooper, 1170
- "Warsaw between the World Wars," by Wynot, 806
- Washington, Booker T., 218
- Wasserstrom, Robert, and Murdo J. MacLeod, editors, "Spaniards and Indians in Southeastern Mesoamerica: Essays on the History of Ethnic Relations" (E), 1432
- Waterbury, John, "The Egypt of Nasser and Sadat: The Political Economy of Two Regimes," 1127
- Waters, Neil L., "Japan's Local Pragmatists: The Transition from Bakumatsu to Meiji in the Kawasaki Region," 827
- Watson, Ian, "Song and Democratic Culture in Britain: An Approach to Popular Culture in Social Movements," 1081
- "The Way of Duty," by Buel and Buel, 1147
- Wayne, Michael, "The Reshaping of Plantation Society: The Natchez District, 1860–1880," 521
- "Weapons Systems and Political Stability," by Quigley, 98
- Weaver, Frederick Stirton, "Class, State, and Industrial Structure: The Historical Process of South American Industrial Growth," 1186
- Webb, George Ernest, "Tree Rings and Telescopes: The Scientific Career of A. E. Douglass," 534
- Weber, David J. (R), 1156
- Weber, David S. (R), 878
- Webster, Charles, "From Paracelsus to Newton: Magic and the Making of Modern Science," 401
- Webster, Graham (R), 743
- Webster, Noah, 202
- Weekes, Brian (R), 158
- Weeks, Louis B., "Kentucky Presbyterians," 1384
- Weigley, Russell F., editor, "Philadelphia: A 300-Year History," 878
- Weil, Simone, 446
- Weiner, Dora B. (R), 132
- Weinstein, Donald (R), 475
- Weir, Robert M., "Colonial South Carolina: A History," 511
- Weisberg, D. Kelly, editor, "Women and the Law: A Social Historical Perspective," vol. 1, "Women and the Criminal Law"; vol. 2, "Property, Family, and the Legal Profession" (E), 571
- Weisberg, Gabriel P., editor, "The European Realist Tradition," 94
- Weisbrot, Robert, "Father Divine and the Struggle for Racial Equality," 542
- Weiss, James Michael (R), 1106
- Weiss, Nancy J., "Farewell to the Party of Lincoln: Black Politics in the Age of FDR," 1404
- Weisser, Henry, "April 10: Challenge and Response in England in 1848," 430
- Weisser, Michael R. (R), 458
- Weissman, Neil B. (R), 812
- Weissman, Ronald F. E. (R), 1107
- Weisz, George, "The Emergence of Modern Universities in France, 1863–1914," 138
- "Die Weizsäcker-Papiere, 1900–1932," edited by Hill, 150
- Welch, David, "Propaganda and the German Cinema, 1933–1945," 466
- Welch, David, editor, "Nazi Propaganda: The Power and the Limitations" (E), 899
- Welch, Richard E., Jr. (R), 526
- "The Well-Ordered Police State," by Raeff, 404
- Weller, Robert P., and Scott E. Guggenheim, editors, "Power and Protest in the Countryside: Studies of Rural Unrest in Asia, Europe, and Latin America" (E), 243
- Wells, Robert V. (R), 201
- Wells, Roger, "Insurrection: The British Experience, 1795–1803," 766
- Wells, Samuel F., Jr., and William H. Becker, editors, "Economics and World Power: An assessment of American Diplomacy since 1789" (E), 1430
- Welter, Barbara (R), 516; 1383
- Wemple, Suzanne (R), 106; 1050
- Werner, Karl Ferdinand, "Vom Frankenreich zur Entfaltung Deutschlands und Frankreichs: Ursprünge, Strukturen, Beziehungen, ausgewählte Beiträge; Festgabe zu seinem sechzigsten Geburtstag" (E), 1428
- West, Delno C. (R), 746
- "West Germany," by Balfour, 156
- West, Robert Craig (R), 217
- "Der Westen und die Sowjetunion," edited by Niedhart (E), 1197
- Westfall, Richard S. (R), 1069
- Westphall, Victor, "Mercedes Reales: Hispanic Land Grants of the Upper Rio Grande Region," 1156
- Wetzles, Walter, and Leonard Schulze, editors, "Literature and History" (E), 572
- Wexler, Imanuel, "The Marshall Plan Revisited: The European Recovery Program in Economic Perspective," 1407
- Wheatley, Paul, and Kernial Singh Sandhu, editors, "Melaka: The Transformation of a Malay Capital, c. 1400–1980," 835
- Wheaton, Barbara Ketcham, "Savoring the Past: The French Kitchen and Table from 1300 to 1789," 450
- Wheeler, Douglas L. (R), 179
- Wheeler, Gerald E. (R), 538
- Wheeler, William Bruce, and Michael J. McDonald, "Knoxville, Tennessee: Continuity and Change in an Appalachian City," 880
- Whelan, Heide W., "Alexander III and the State Council: Bureaucracy and Counter-Reform in Late Imperial Russia," 169
- "When Fathers Ruled," by Ozment, 1295
- Whisenhunt, Donald W. (R), 839
- Whisnant, David E., "All That Is Native and Fine: The Politics of Culture in an American Region," 1183
- Whisnant, David E., "Modernizing the Mountaineer: People, Power, and Planning in Appalachia," 231
- Whitaker, Reginald (R), 557

- White, Eugene Nelson, "The Regulation and Reform of the American Banking System, 1900-1929," 217
- White, Gordon, *et al.*, editors, "Revolutionary Socialist Development in the Third World" (E), 242
- White, Hayden (R), 1037
- "White Land, Black Labor," by Flynn, 1161
- Whitehouse, David, and Richard Hodges, "Mohammed, Charlemagne, and the Origins of Europe: Archaeology and the Pirenne Thesis," 1061
- Whitnah, Donald R. (R), 1412
- Whitten, David O., "The Emergence of Giant Enterprise, 1860-1914," 1400
- "Who Owns the Wildlife?" by Tober, 855
- "Who Voted?" by Kleppner, 214
- Whyte, Ian D. (R), 1070
- Whyte, Martin King (R), 1374
- Wiarda, Howard J. (R), 564
- Wickberg, Edgar (R), 886
- Wickwire, Franklin B. (R), 428
- Wiener, Joel H., "Radicalism and Freethought in Nineteenth-Century Britain: The Life of Richard Carlile," 432
- Wiener, Jonathan M. (R), 1161
- Wiese, Carl, "Expedition in East-Central Africa, 1888-1891: A Report," 1131
- Wild, Trevor, editor, "Urban and Rural Change in West Germany" (E), 899
- "Wildlife and Man in Texas," by Doughty, 1180
- Wildman, Allan K. (R), 492
- Wilken, Robert L., "John Chrysostom and the Jews: Rhetoric and Reality in the Late 4th Century," 1059
- Wilkins, Mira (C), 586
- "Will and Circumstance," by Hampson, 1084
- William II, 1319
- "William Jennings Bryan," by Clements, 220
- "William Molyneux of Dublin, 1657-1698," by Simms, 129
- "William of Auvergne and Robert Grosseteste," by Marrone, 748
- "William Rufus," by Barlow, 1319
- William Shakespeare and the American People: A Study in Cultural Transformation*, by Levine, 34-66
- Williams, Aubrey Willis, 224
- Williamson, Harold F., and Michael W. Sedlak, "The Evolution of Management Education: A History of the Northwestern University J. L. Kellogg Graduate School of Management, 1908-1983," 1409
- Williamson, Samuel R., Jr. (R), 456
- Williamson, Samuel R., Jr., and Peter Pastor, editors, "Essays on World War I: Origins and Prisoners of War" (E), 242
- Willis, F. Roy (R), 156; 1303
- Willkie, Wendell, 1405
- Wilson, Constance M. (R), 834
- Wilson, Leonard G. (R), 128
- Wilson, Raymond, "Ohiyesa: Charles Eastman, Santee Sioux," 858
- Wilson, Robert J., III, "The Benevolent Deity: Ebenezer Gay and the Rise of Rational Religion in New England, 1696-1787," 1385
- Wilson, Stephen, editor, "Saints and Their Cults: Studies in Religious Sociology, Folklore, and History" (E), 1426
- Wilson, Woodrow, 1170
- "Winds of History," by Backer, 796
- "The Winged Gospel," by Corn, 876
- Winkel, Harald (R), 795
- Winkler, Heinrich August, editor, "Nationalismus in der Welt von Heute" (E), 241
- "Winston Churchill's World View," by Thompson, 125
- "Wirtschaftliche Integration im 19. Jahrhundert," by Hahn, 146
- Wise, Stephen S., 225
- "Wissenschaft, Staat, Mäzene," by Alter, 123
- Withers, Charles W. J. (R), 127
- Withey, Lynne, "Urban Growth in Colonial Rhode Island: Newport and Providence in the Eighteenth Century," 1386
- Withrington, Donald J., editor, "Shetland and the Outside World, 1469-1969" (E), 897
- Wittig, Peter, "Der englische Weg zum Sozialismus: Die Fabier und ihre Bedeutung für die Labour Party und die englische Politik," 770
- Wodarski, Monsignor John P., 253
- Wohl, Anthony S. (R), 769
- Wohl, Robert (R), 1087
- Wolf, Eric R., "Europe and the People without History," 89
- Wolfe, James H. (R), 154
- Wolfskill, George (R), 868
- Woloch, G. Michael (R), 1058
- "Wolves for the Blue Soldiers," by Dunlay, 208
- "Woman's Place Is at the Typewriter," by Davies, 1165
- "Women and American Socialism, 1870-1920," by Buhle, 212
- "Women and Slavery in Africa," edited by Robertson and Klein, 1370
- "Women and the Criminal Law," vol. 1 of "Women and the Law," edited by Weisberg (E), 571
- "Women and the Law," vol. 1, "Women and the Criminal Law"; vol. 2, "Property, Family, and the Legal Profession," edited by Weisberg (E), 571
- "Women in the Middle Ages," by Lucas, 747
- "Women of the English Renaissance and Reformation," by Warnicke, 421
- WOMEN'S HISTORY: Altschuler and Saltzgeber, "Revivalism, Social Conscience, and Community in the Burned-Over District," 204; Atkinson, "Mystic and Pilgrim," 749; Bacchi, "Liberation Deferred?" 236; Baker, *The Domestication of Politics: Women and American Political Society, 1780-1920*, 620-47; Bidelman, "Pariahs Stand Up!" 136; Boyd and Brackenridge, "Presbyterian Women in America," 862; Brenzel, "Daughters of the State," 1169; Buel and Buel, "The Way of Duty," 1147; Buhle, "Women and American Socialism, 1870-1920," 212; Clinton, "The Plantation Mistress," 196; Davies, "Woman's Place Is at the Typewriter," 1165; Dudden, "Serving Women," 206; Eckhardt, "Fanny Wright," 1393; Engel, "Mothers and Daughters," 809; Gorham, "The Victorian Girl and the Feminine Ideal," 1329; Holum, "Theodosian Empresses," 415; Kessler-Harris, "Out to Work," 193; Lucas, "Women in the Middle Ages," 747; Matthaëi, "An Economic History of Women in America," 215; McLaren, "Sexuality and Social Order," 781; McNamara, "A New Song," 1316; Minault, editor, "The Extended Family," 833; Motz, "True Sisterhood," 1383; Moynihan, "Rebel for Rights," 860; Mullaney, "Revolutionary Women," 1048; Norton, *The Evolution of White Women's Experience in Early America*, 593-619; Offen, *Depopulation, Nationalism, and Feminism in Fin-de-Siècle France*, 648-76; Peterson, *No Angels in the House: The Victorian Myth and the Paget Women*, 677-708; Robertson and Klein, editors, "Women and Slavery in Africa," 1370; Robins-Mowry, "The Hidden Sun," 501; Rotella, "From Home to Office," 1165; Ryan, "The Empire of the Mother,"

- 205; Sachs, "The Invisible Farmers," 548;
Scarborough, "Belle Boyd," 853; Sealander, "As
Minority Becomes Majority," 547; Sievers, "Flowers
in Salt," 1137; Smith, *The Contribution of Women to
Modern Historiography in Great Britain, France, and the
United States, 1750–1940*, 709–32; Strauss,
"Traitors to the Masculine Cause," 97; Sweet,
"The Minister's Wife," 516; Taylor, "Eve and the
New Jerusalem," 121; Warnicke, "Women of the
English Renaissance and Reformation," 421; Yao,
"Chinese Women," 826
- Wood, Charles T. (R), 1319
- Wood, Neal, "The Politics of Locke's Philosophy: A
Social Study of 'An Essay Concerning Human
Understanding,'" 735
- Wood, Peter H., *La Salle: Discovery of a Lost Explorer*,
294–323
- Wooden, Warren W., "John Foxe," 758
- Woodside, Alexander (R), 185
- Woodside, Alexander, and David K. Wyatt, editors,
"Moral Order and the Question of Change: Essays
on Southeast Asian Thought" (E), 251
- Woodson, Carter G., 1005
- Woodward, David R., "Lloyd George and the
Generals," 774
- Woody, Robert H., and E. Stanly Godbold, Jr.,
"Christopher Gadsden and the American
Revolution," 515
- Wootton, David, "Paolo Sarpi: Between Renaissance
and Enlightenment," 1109
- "Work, Community, and Power," edited by Cronin
and Sirianni (E), 568
- "Workers Control and Socialist Democracy," by
Sirianni, 1122
- "Working for the Railroad," by Light, 864
- "Workingmen's Democracy," by Fink, 864
- "The World of the Mugglestonians," by Hill *et al.*, 426
- WORLD WAR I: Cobb, "French and Germans, Germans
and French," 140; Lieven, "Russia and the Origins
of the First World War," 1362; Prior, "Churchill's
World Crisis as History," 125; Sanders and Taylor,
"British Propaganda during the First World War,
1914–18," 1081; Siegelbaum, "The Politics of
Industrial Mobilization in Russia, 1914–17," 1360;
Stevenson, "French War Aims against Germany,
1914–1919," 456
- WORLD WAR II: Baily, "Faint Praise," 869; Boog *et al.*,
"Das Deutsche Reich und der Zweite Weltkrieg,"
vol. 4, "Der Angriff auf die Sowjetunion," 1353;
Cobb, "French and Germans, Germans and
French," 140; de Jong, "Het Koninkrijk der
Nederlanden in de Tweede Wereldoorlog," vol.
10b, "Het laatste jaar II," 460; de Montclos, "Les
chrétiens face au nazisme et au stalinisme," 1306;
Glees, "Exile Politics during the Second World
War," 127; Herde, "Pearl Harbor, 7. Dezember
1941," 540; Irons, "Justice at War," 871; Molin,
"Hemmakriget," 156; Nissen, editor, "Scandinavia
during the Second World War," 1092; Tiushkevich
et al., "Istoriia vtoroi mirovoi voiny, 1939–1945,"
vol. 12, "Itogi i uroki vtoroi mirovoi voiny," 494
- Wren, Christopher, 118
- Wright, Fanny, 1393
- Wright, Gavin (R), 1397
- Wright, Gordon (R), 140
- Wright, Thomas C. (R), 1192
- Wright, William E., editor, "Austria since 1945" (E),
574
- "Writings on South Asian History and Society," vol. 2
of "Subaltern Studies," edited by Guha (E), 1430
- Wurgaft, Lewis D., "The Imperial Imagination:
Magic and Myth in Kipling's India," 831
- Wyatt, David K., and Alexander Woodside, editors,
"Moral Order and the Question of Change: Essays
on Southeast Asian Thought" (E), 251
- Wynia, Gary W. (R), 893
- Wynot, Edward D., Jr., "Warsaw between the World
Wars: Profile of the Capital City in a Developing
Land, 1918–1939," 806
- Wythe, George, 514
- Yamauchi, Edwin M. (R), 1055
- Yaney, George, "The Urge to Mobilize: Agrarian
Reform in Russia, 1861–1930," 1118
- Yang, Anand A. (R), 832
- Yao, Esther S. Lee, "Chinese Women: Past and
Present," 826
- Yarshater, Ehsan, editor, "The Cambridge History of
Iran," vol. 3, "The Seleucid, Parthian, and Sasanian
Periods," 1055
- Yavetz, Zwi, "Julius Caesar and His Public Image,"
741
- Y'Blood, William T., "Hunter-Killer: U.S. Escort
Carriers in the Battle of the Atlantic," 1406
- Yellowitz, Irwin (R), 1400
- Yellowitz, Irwin (R), 528
- "Yesterday's Soldiers," by Nunn, 239
- Yoshitsu, Michael M., "Japan and the San Francisco
Peace Settlement," 1377
- Youngs, Joyce (R), 760
- Young, David (R), 1166
- Young, Jordan (R), 1421
- "The Young Lukács," by Congdon, 482
- Young, Mary (R), 1390
- "The Young Richelieu," by Marvick, 1333
- "The Younger Pitt," by Ehrman, 766
- Youngson, A. J. (R), 768
- Yserns, Rafael Maroto, 1192
- Yudelman, David, "The Emergence of Modern South
Africa: State, Capital, and the Incorporation of
Organized Labor on the South African Gold Fields,
1902–1939," 181
- Zachariadou, Elizabeth A., "Trade and Crusade:
Venetian Crete and the Emirates of Monteshe and
Aydin, 1300–1415," 1126
- Zangrando, Robert L. (R), 1411
- Zapantis, Andrew L., "Greek-Soviet Relations,
1917–1941," 478
- "Das zarische Russland und der Erster Weltkrieg," by
Linke, 814
- Zarnowski, Janusz, editor, "Dictatorships in
East-Central Europe, 1918–1939" (E), 1430
- Zasulich, Vera, 814
- Zebel, Sydney H. (R), 1077
- Zeckhauser, Richard J., and Peter D. McClelland,

- "Demographic Dimensions of the New Republic: American Interregional Migration, Vital Statistics, and Manumissions, 1800–1860," 201
- "Das Zeitalter der Aufklärung in der Provinz Quito," by Keeding, 1419
- "Zionism and Arabism in Palestine and Israel," edited by Kedourie and Haim, 497
- Zitomersky, Joseph, editor, "On Making Use of History: Research and Reflections from Lund" (E), 249

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1(a)

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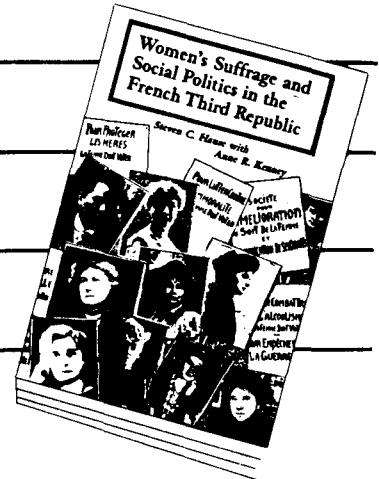
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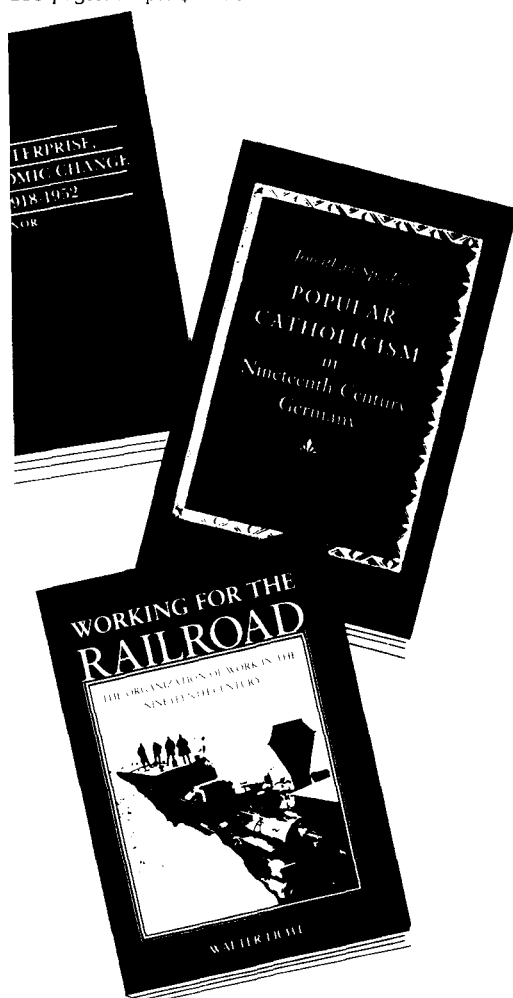
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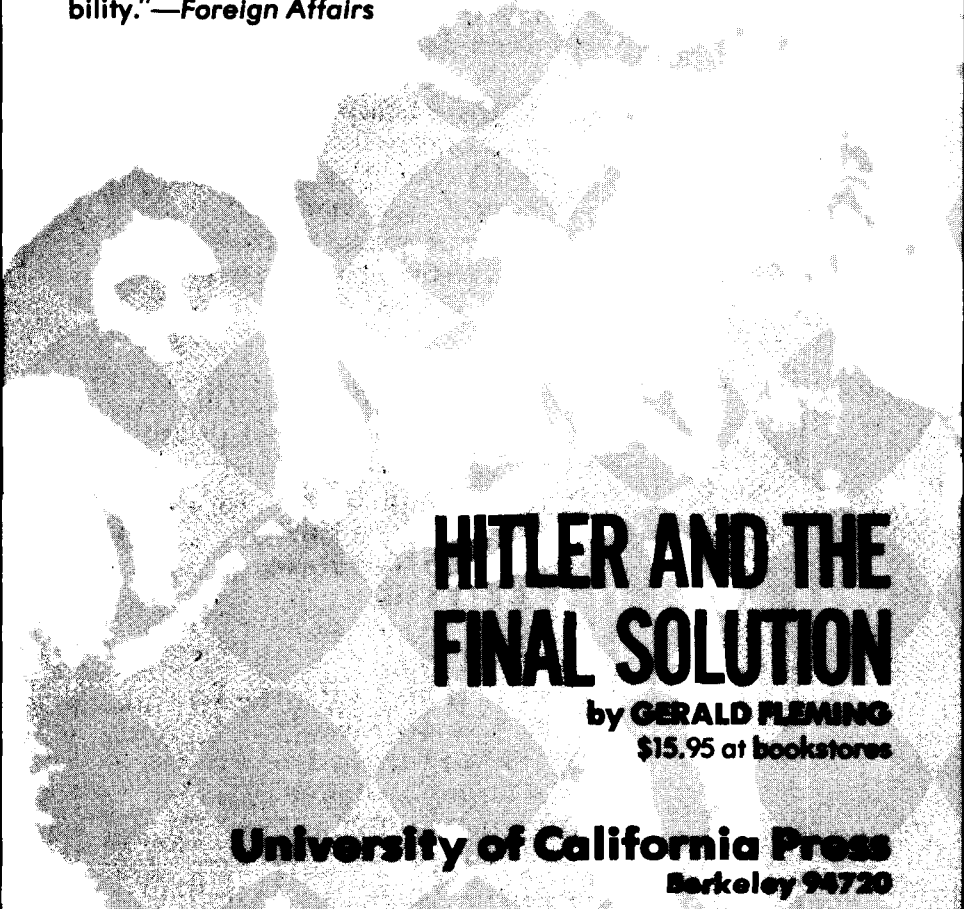
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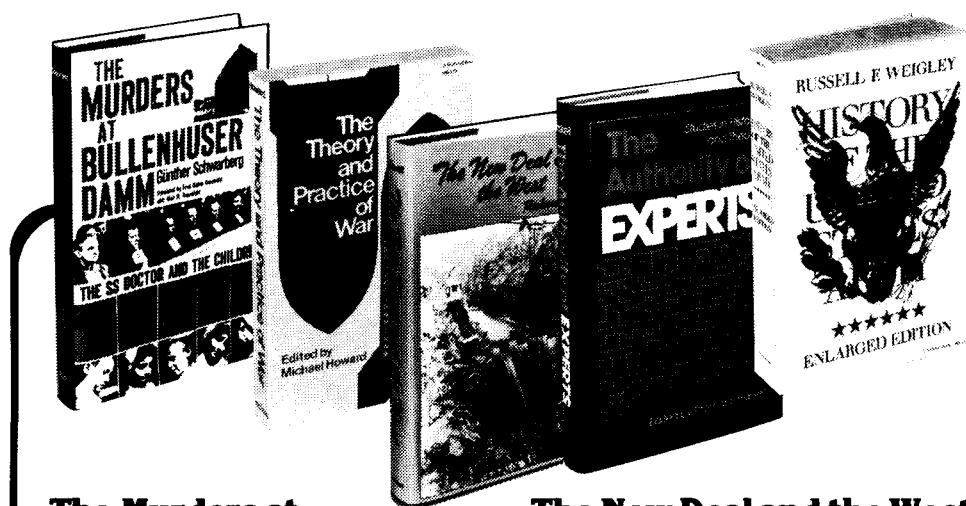
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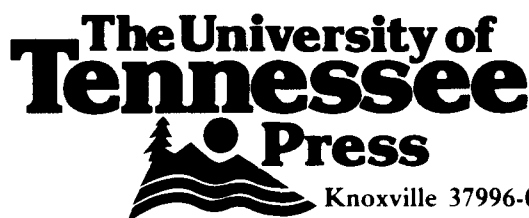
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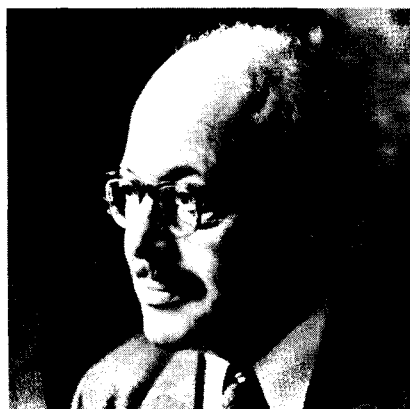
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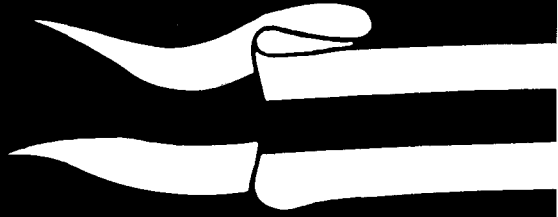
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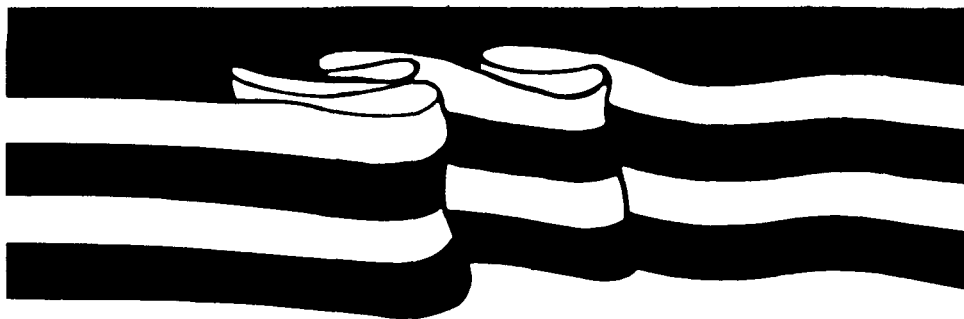
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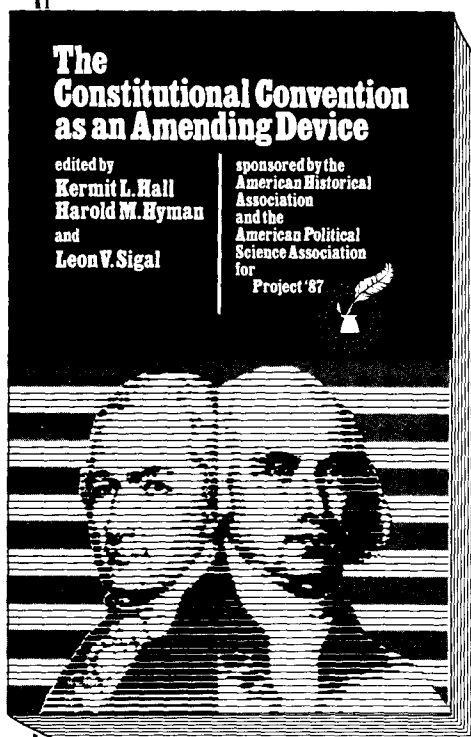
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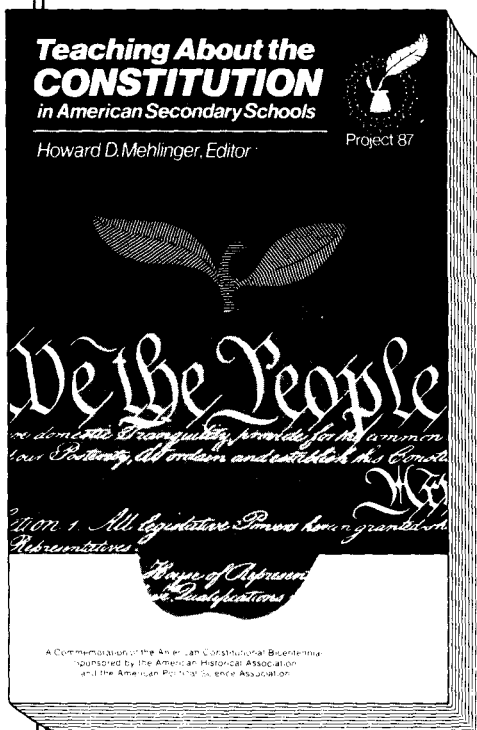
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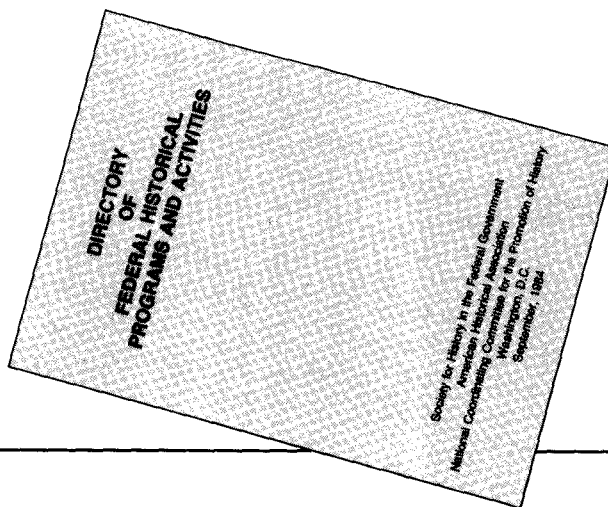
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Columbia University Press	4–5	Routledge & Kegan Paul	33
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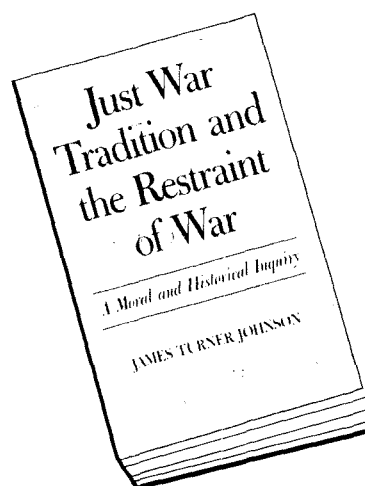
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